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The Journal of HEREDITY

A monthly publication devoted to Plant Breeding Animal Breeding and Eugenics

ROOT-BORNE FRUIT OF ROXBURGH'S FIG

(See Page 533)

Hereditary Nomadism — **Eliminating Mental Defects**

Index to Volume 18

DECEMBER, 1927 — VOL. 18, NO. 12

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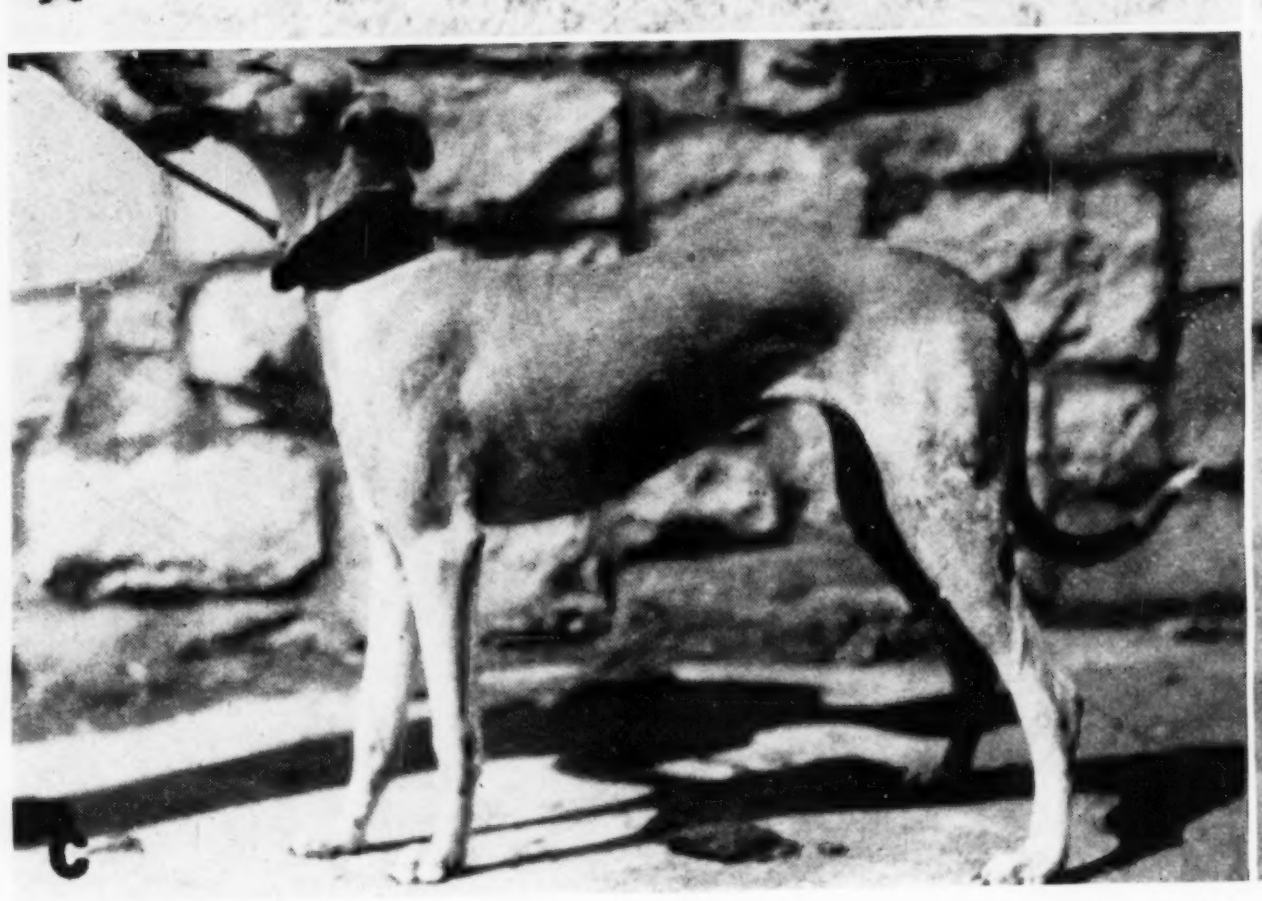


GREYHOUND COLORS

Frontispiece

A—Meadows Imp., red. *B*—Mon ami, black. *C*—Lena Meadows, fawn. *D*—Glen Rover, blue. It is a curious fact that while greyhounds are a variety of colors, none of them are grey. Black and brindle are most frequently encountered. Blue appears to be black modified by a diluting factor.

COAT COLOR INHERITANCE IN GREYHOUNDS

D. C. WARREN
Geneticist, Kansas State Agricultural College, Manhattan

GREYHOUNDS are one of the oldest known breeds of dogs. Animals very similar to the present day Greyhound are figured on the monuments of ancient Egypt. This breed has been common from the earliest historic times in Asia and generally throughout Europe. Dogs of this type were quite early employed for hunting. In their breeding, special attention has been given to slenderness and power of endurance in running. This breed of dogs has been very popular for coursing in England where the hare is used. Coursing has recently also become a popular sport in the western portion of the United States where the native jack rabbit is used. Hounds bred for this purpose supplied the material for this study.

The term Greyhound is somewhat of a misnomer since, although several color variations are found in this breed, grey is not among them. There is, of course, a blue color which approaches a grey, but blues are less common than some of the other colors.

Material and Methods

The data presented were taken from the Greyhound Stud Books,* volumes 11 to 19. These records are published by the National Coursing Association. The writer appreciates the fact that stud book records are subject to a certain amount of inaccuracy since the descriptions presented for registration cannot be verified. However, it is believed that in cases where the numbers are large enough such records can be used for fairly accurate analysis of the genetic relations. In situations where certain colors or types are at a premium, wilful deceit might be a factor in vitiating the results. In the National Coursing Association, a Greyhound is valued exclusively for its running ability and as far as the writer could learn from conversation with numerous breeders, there exist no prejudices or preferences with regard to color. In compiling data for this study, litters of less than three were not considered. A card was made for each mating and the cards were organized according to the colors or patterns involved in the mating. Seven hundred thirty-three matings and nearly four thousand individuals are included in the data presented.

Colors

Five solid colors and two patterns are found in this breed of dogs. The colors are black, blue, red, fawn, and white. The two patterns found are brindle and white spotting. All individuals observed by the writer possessed black muzzles indicating that all colors carry black.

Black.—Greyhounds which are entirely black are seldom found. Usually they have a white star on the breast and the feet, and tip of the tail frequently shows some white. The amount of white found is, however, sufficiently limited to make it improbable that this pattern would be confused with pibald spotting which is later discussed. This marking of the extremities with white was found in all colors but the stud book descriptions were not sufficiently complete to make it possible to determine whether this be a heritable variation.

*Acknowledgment is due Mr. C. F. Horne, Secretary of the National Coursing Association for kindly providing records from which material for this study was obtained.

Frontispiece

A—Meadows, fawn. *B*—Mon ami, black. *C*—Lena Meadows, red. *D*—Glen Rover, blue. It is a curious fact that while greyhounds are a variety of colors, none of them are grey. Black and brindle are most frequently encountered. Blue appears to be black modified by a diluting factor.

Blue.—Dogs of this color are marked much like the blacks except that the coat color is a bluish gray. This color is quite distinct from black and not likely to be confused with any other color in the description.

Red.—The color here is a brownish-red but never dark enough to be confused with the liver color common in other breeds but not found in Greyhounds.

Fawn.—The typical fawn is of a faded yellowish color. All gradations are found between the red and fawn, and some of the intermediate colors present difficulties in classification.

White.—Entirely white individuals are seldom found and all white Greyhounds show some pigment on the muzzle and eyes. It is also probable that some individuals listed as white may carry a small amount of color which is disregarded in registration. As will be given later in the discussion, it is the belief of the writer that there is no such genotype as self white in Greyhounds.

Brindle.—This pattern in its varying degrees is probably the most common color type in Greyhounds. It is constituted of irregular streaks of black upon the fawn and red ground colors. The amount of brindling varied from instances where there were only faint traces to those in which the brindled portion of the pattern predominated making the animal quite dark.

White spotting.—White spotting is also quite a common pattern in Greyhounds. It acts upon all self colors and is found combined with brindling. The amount of white varies from very small areas to instances in which the animal is almost entirely white.

Data

In the study of genetic relationships of the various colors, dogs showing spotting were not included since cases where the white predominated might make accurate classification of the color difficult. Thus, the data presented in Table I do not indicate matings in which either parent was classified as spotted. In the original organization of the data the sexes were separated. Also note was made of the sex of the parent through which a particular color was introduced. Examination of the data organized in this manner showed no evidence of sex linkage of any of the factors involved. So for the sake of brevity the two sexes are combined in all tables.

Table I shows the data from all crosses in detail while in Table II. all spotted individuals are included under the color which was characteristic of their non-white areas.

Black

As in the case in other mammals, black is either dominant or epistatic to all other colors. Black crossed by black gives mostly blacks, but brindles, reds and fawns all segregated from such a cross, indicating that these colors were either recessive or hypostatic to black. When blacks were crossed to brindles there resulted about equal numbers of blacks and brindles together with a few reds, fawns and blues. These results combined with those from the cross of brindle by brindle from which practically no blacks were obtained, may be taken to indicate that black is either dominant or epistatic to reds and fawns. Then, providing black and brindle are allelomorphs, crosses between black and brindle should give no reds or fawns unless the three compose a triple allelomorphic series. It is seen that crosses of black by brindle produce blacks, brindles and fawns or reds indicating, as is later more fully considered, that black, brindle, and fawn or red are in a triple allelomorphic series.

Brindle

The relationship of brindle to black has already been discussed. From Table II it will be seen that crosses of brindle to fawn or red produce primarily brindles, reds and fawns. The crosses in which both parents are

fawns or reds produce none or very few brindles. This may be taken to indicate that brindle is either epistatic or dominant to red and fawn. The few brindles resulting from crosses involving only reds or fawns are probably due to errors in recording the color of the parents or offspring. Crosses of brindle by brindle gave offspring which were chiefly brindle, fawn and red. The few exceptional blacks are probably due to errors in records. The few blues and whites will be explained in later discussion. The ratios obtained from this cross further indicate the existence of an allelomorphic series.

Fawn and Red

The colors red and fawn are treated together for two reasons: first, in many instances it is difficult to distinguish between the two; and second, the two react in quite a similar manner toward other colors. The typical fawns and reds are quite distinct from one another but there exist intermediate grades which are difficult to classify. This difficulty of classification is probably a source of some degree of error in the records.

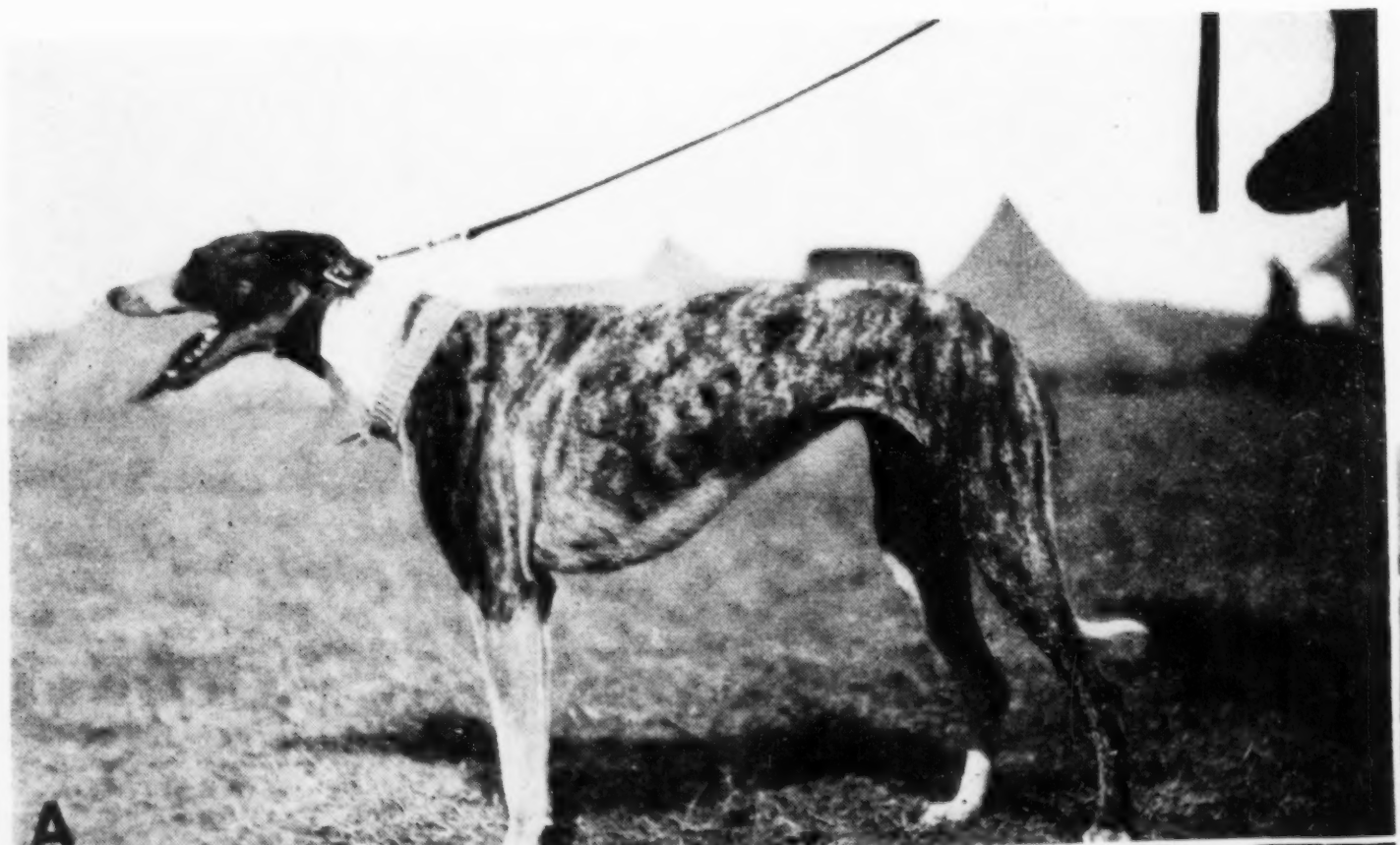
Since we have this gradation of red and fawn and also the existence of blue hounds, one is at once lead to suspect that we have here the recessive dilution factor well known in other mammals. If fawn is a dilute red, crosses of fawn by fawn should produce only fawn dogs. These matings produced 6 brindles, 33 fawns and 14 reds. There is a preponderence of fawns, but the occurrence of any reds is not to be expected. The failure to realize the expected results may be due to the difficulty of distinction between reds and fawns. From crosses of red by fawn about equal numbers of fawns and reds were secured, while crosses of red by red produced a considerable preponderance of reds, 30 reds to 8 fawns. So from the data available it is impossible to state definitely that fawn is the result of a dilution factor acting upon red. Since practically nothing except fawns or reds were obtained from crosses involving only these two colors it would seem that one is the result of some modifying factor acting upon the other. Both red and fawn behave either recessively or are hypostatic to black and brindle.

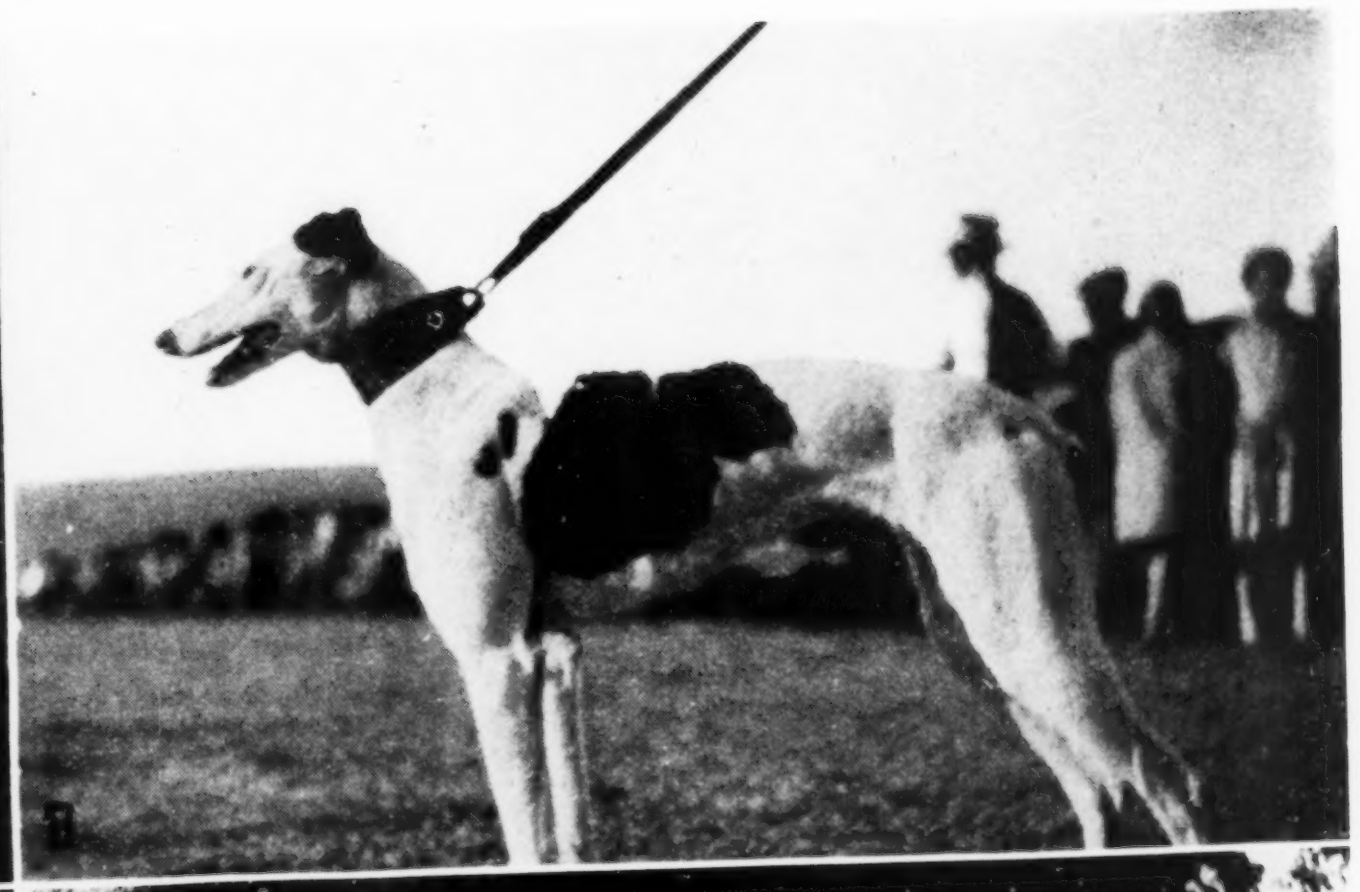
Multiple Allelomorphic Series

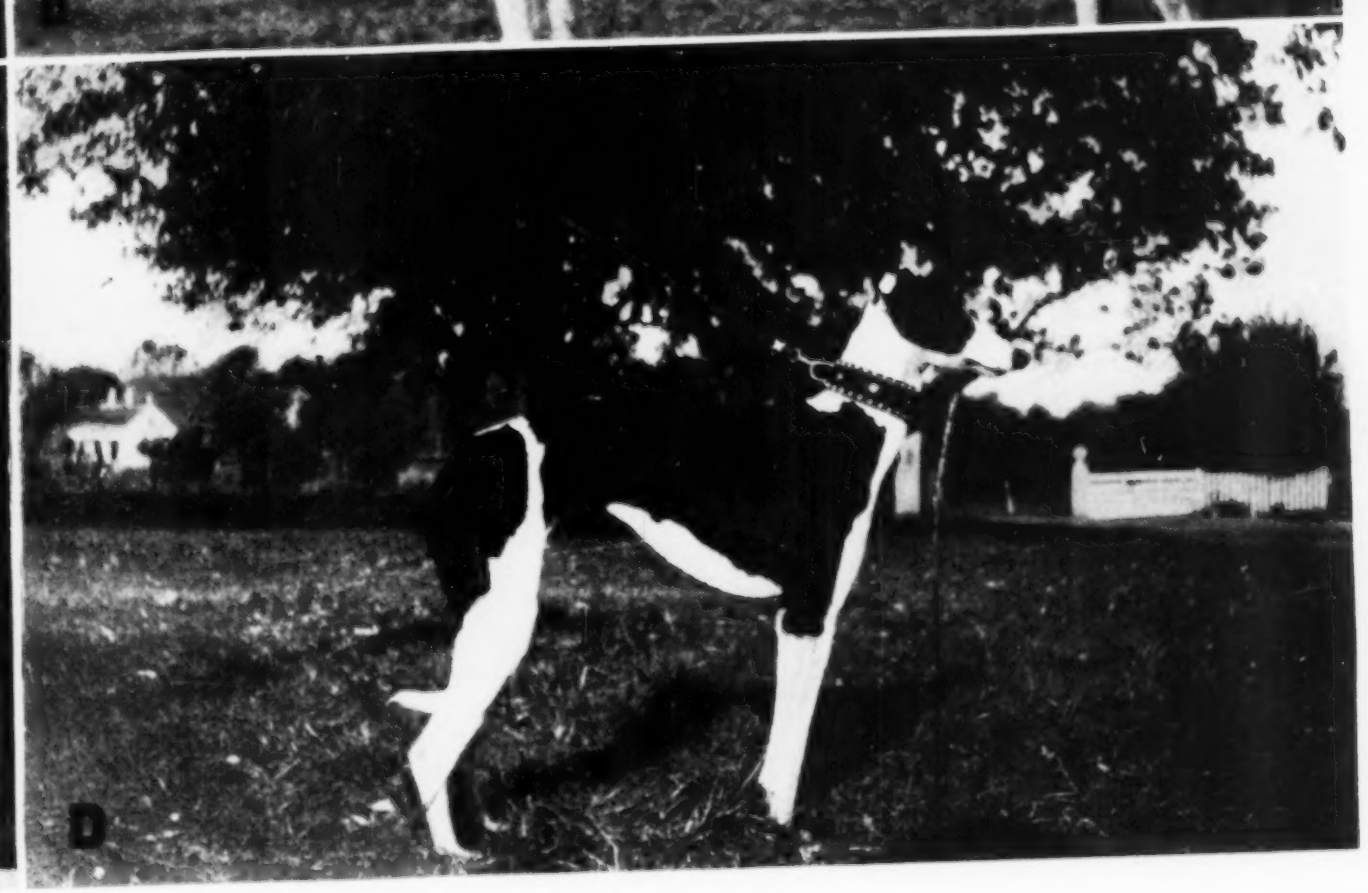
Little[1] has suggested that in Great Danes black, brindle and fawn constitute a triple allelomorphic series, black representing the completely extended condition, brindle the partially extended, and fawn the non-extended. He found black to be dominant to brindle and fawn, and brindle to be dominant to fawn. Thus, if E indicates black, E′ brindling and e fawn; blacks could be of the constitution EE, EE′ and Ee, brindles E′E′ and E′e, and fawns ee.

In Table III are found data upon the relations of black, brindle and fawn taken from my own results for Greyhounds and those of Little upon Great Danes. Since in the Great Danes, no red was found and since red and fawn appear to be upon the same major factorial basis in Greyhounds, in my own data the red and fawn classes were combined under the heading of fawn. It is seen that the ratios in all cases are quite similar, indicating that the three colors are upon the same factorial basis in these two breeds of dogs.

It has been suggested by Little that in Great Danes these three patterns comprise a multiple allelomorphic series. From data obtained from stud books it is somewhat difficult to determine certainly that the results might not be explained upon some other basis, since nothing is known of the homozygous and heterozygous relations of the parents used. The distribution shown by my own data and that Little in Table III might also be explained by the assumption that the three characters black, brindle and fawn all belong to different linkage groups, with black epistatic to brindle and fawn, and brindle epistatic to fawn. However, the fact, that by defi-







BRINDLE AND SPOTTED GREYHOUNDS

Figure 1

A—May Echo, dark brindle. *B*—Rose Bud Bell, black and white spotted—predominantly white. *C*—Golden May, light brindle. *D*—Runner Up, spotted—predominantly black. Black, brindle and fawn appear to constitute an allelomorphic series, the colors being arranged in the order of their dominance. Predominantly black spotting is dominant over spotting containing more white.

nition no individual can carry more than two factors of an allelomorphic series, may be utilized to determine whether we are here dealing with an example of independent assortment or an allelomorphic series. So in crosses of black to fawn or red, if we are dealing with an allelomorphic series, the blacks would be of one of three different genotypes EE, EE′ or Ee. The first genotype (EE) would produce all blacks, the second (EE′) would produce only blacks and brindles and the third (Ee) would produce only blacks and fawns or reds. The distribution of the offspring of this third type of black should indicate whether we are dealing with a case of independent assortment or an allelomorphic series. If the three factors belong to different linkage groups, then it would be possible to obtain from a single mating of black to fawn, blacks, brindles and fawns or reds. If these three colors constitute an allelomorphic series, then from such a cross brindles could not appear in the same mating with fawns and reds. To answer this question the writer has analyzed the matings of black by fawn and red. Out of 40 matings of black by red or fawn, 10 gave all black offspring, 15 gave only black and brindle and 12 gave only black and fawn or red. In only three matings were brindles found occurring with fawns or reds, and it seems quite possible that this case may be due to error in the records. Even if we include the cases of occurrence of brindles with fawns the percentage of brindles is not as great as would be expected from independent assortment. It therefore, seems justifiable to assume that we have also in Greyhounds an allelomorphic series of black, brindle and fawn, the foregoing arrangement being in the order of their dominance.

Blue

The color blue is well known in other mammals and has usually been found to be due to a recessive dilution factor acting upon black. Although black is a very common color among hounds, and blue is found crossed with most other colors, it chanced that not many matings of black by blue were recorded. As far as the writer could determine, no factor other than chance was responsible for this shortage. The small amount of data obtained from this cross seem to indicate that black is dominant to blue. However, the available data offer no indication that the same dilution factor changes black to blue and red to fawn. If blue and fawn be due to the same diluter, then the theoretical expectation from crosses of blue by fawn would include neither reds nor blacks. The results of this cross were 24 blacks, 18 brindles, 20 fawns, 1 red and 5 blues. The cross of fawn by fawn produced no blacks so it seems that the blue parent is responsible for the blacks. Granting the assumption that blue is recessive to black, the black offspring are difficult to account for and it hardly seems that the inconsistency is due to confusion of blue and black since in most cases the colors appear to be quite distinct. So, although it is possible that the blue of Greyhounds is due to a recessive diluter, it seems improbable that the same factor is responsible for both blue and fawn.

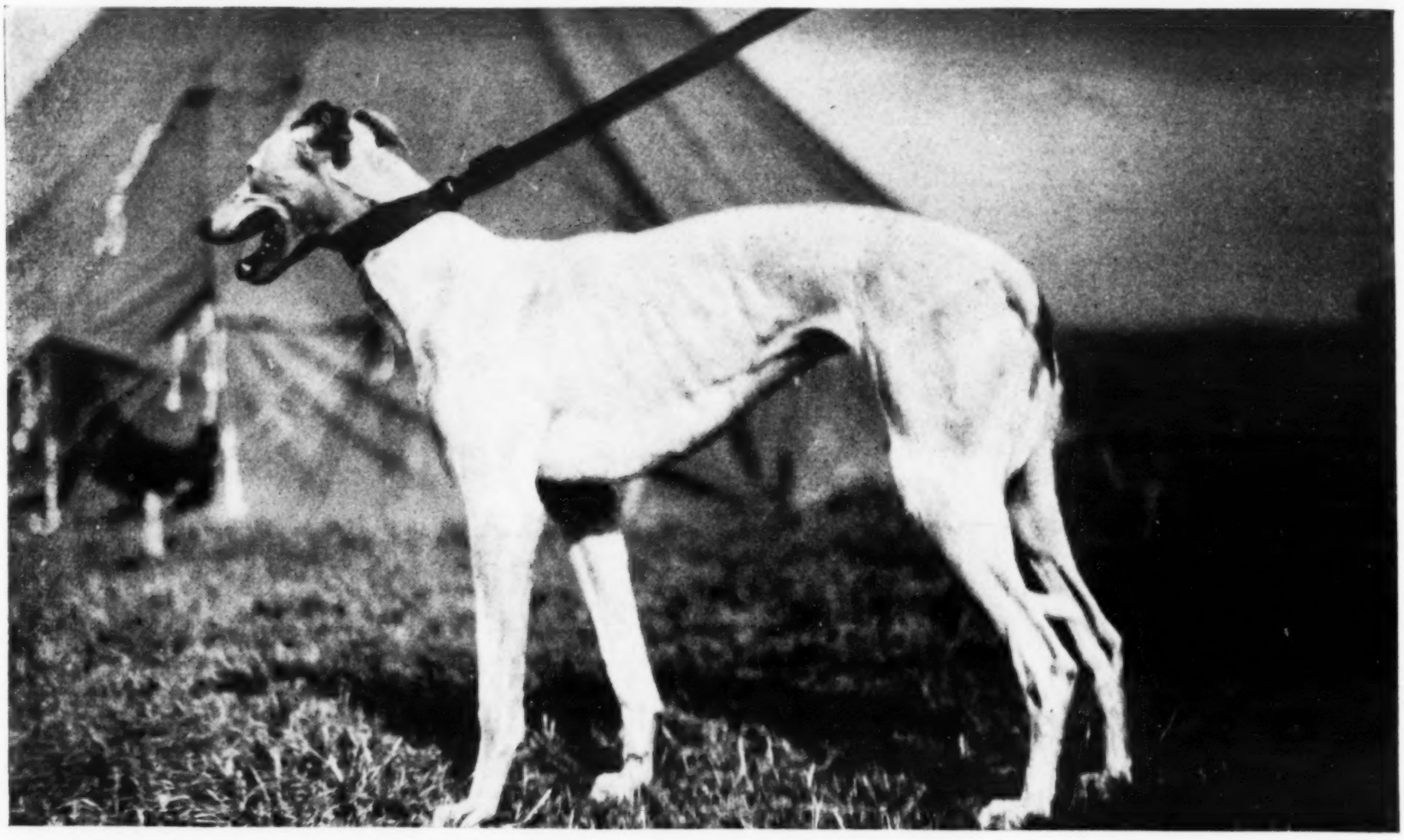
White

An all-white individual is seldom recorded, and although crosses of white by practically all other colors were listed, very few of the offspring are described as white. It is also noted (Table I) that from crosses in which one parent was white, a large number of spotted offspring resulted. Crosses of black by white produced only 6 white out of 85 offspring; white by brindle, 4 out of 91; white by fawn, 6 out of 60; white by red, none out of 36; and white by blue, no whites appeared among 38 young. In all of these matings practically half of the offspring were spotted. This leads one to suspect that dogs described as white are really spotted ones in which the colored area is reduced to a minimum.

BRINDLE AND SPOTTED GREYHOUNDS

Figure 1

A—May Echo, dark brindle. *B*—Rose Bud Bell, black and white spotted—predominantly white. *C*—Golden Rummer Up, spotted—predominantly black. Black, brindle and fawn appear to constitute an allelomorphic series, the colors being arranged in the order of their dominance. Predominantly black spotting is dominant over spotting containing more white.



EXTREME WHITE SPOTTING

Figure 2

Self white in greyhounds does not seem to exist—the few white individuals apparently being cases of extreme white spotting. Note that in this white individual the ears are pigmented—in nearly all cases white greyhounds show small pigmented areas.

The relation of white to spotting will be discussed in the next division of the paper.

White Spotting

Except for brindling and white spotting, no color patterns other than self are found in Greyhounds. White spotting is very common and the extent of the white area varies from individuals which are practically all white to those which show but a very small spot of white. The spotting seems to assume no particular pattern. Spotting of this character has been found in other mammals and usually reacts recessively to self color. In Greyhounds it has been found that the white spotting is also of this nature. In Table IV are presented the data upon this color pattern In this table are shown the results upon spotting where the parents were black or brindle. These were the two most common colors and most strongly contrasted with white. In Table V are given the data upon spotting, combining the results of crosses involving all colors.

Where white spotted dogs were mated to white spotted the offspring were practically all spotted or white. There resulted a few self colored offspring, but these are probably genotypically spotted showing a minimum of white. That most of these self colored individuals are the result of crossing dogs exhibiting a small amount of white will later be shown in Table VI. The results of the crossings of self to self and self to spotted indicate that white spotting behaves as a recessive to self color.

Under the discussion of white color it was suggested that the recorded self whites were really white spotted individuals exhibiting a maximum of white. The data when organized for the analysis of white spotting (Tables IV and V) seem to substantiate this suggestion. In Tables IV and V it will be seen that the results of matings of white by self are quite similar to those of white spotted by self. The same is true of crosses of spotted by white and spotted by spotted. It there-

fore seems that white spotting is recessive to self color and that self whites are segregates which are genotypically white spotted, showing a maximum amount of white. With regard to the amount of white, it will be shown later that the self white pattern behaves much like that of white spotted dogs upon which the white area exceeds the colored.

In Table VI are shown matings of dogs exhibiting different relative amounts of the white area. Here again, only those matings is which the colored area was black or brindle were utilized. In the stud books the descriptions indicate, in the case of spotting, whether the dogs are predominately white or predominately colored. It was possible therefore, to analyze the mode of inheritance of the degree of white spotting. However, since they were divided only into the two classes, a certain amount of error will occur. From Table VI it is plainly seen that the degree of white spotting is heritable. In cases where spotted dogs, more white than colored, are crossed to similar mates, the offspring are practically all spotted and show white areas similar in extent to that of the parents. When dogs which were predominately white were mated to ones which were more colored than white, a majority of the offspring were in the classes of predominately or entirely colored, the smaller group being predominately white or self white. This indicates that the greater degree of white is recessive to the lesser degree. It is seen that in crosses of spotted by spotted, self colored individuals appear only when dogs more colored than white are involved in the mating. When both parents exhibit more white than colored areas, no self colored dogs appeared among the offspring. Thus it would seem that these selfs are probably spotted dogs showing a minimum of white. That whites are really white spotted individuals showing the white area in its extreme extension is clearly shown in Table VI. Here the whites produce ratios very similar to those resulting from predominately white crossed in a similar manner. The fact that self whites produced a higher percentage of whites might be taken to indicate that additional factors contribute to the production of the extreme degree of white. No data were available from crosses of white by white.

General Discussion

Neither from the history of their origin nor from characteristics, other than color, is there any evidence that Greyhounds and Great Danes are related. However, their color patterns are quite similar in many respects. Blacks, brindles, fawns and blues are found in each breed. The red in Greyhounds also seems to be similar to the undiluted fawn in Great Danes. White spotting in these two breeds differ since in Greyhounds it is of a pibald nature and in Great Danes we have the harlequin pattern.

Black, brindle and fawn seem to be upon the same genetic basis in the two breeds, constituting an allelomorphic series. Blue and light fawn were held by Little to be due to the same dilution factor in Great Danes while in Greyhounds the writer found evidence that this was not the case. Although it seems probable in Greyhounds that differences between black and blue, and red and fawn are due to modifying factors, the available data will not permit the assumption that the same dilution factor is responsible for both.

Little has also suggested that the dilution factor acts upon the brindle pattern producing light brindles. The records upon Greyhounds were not so made that distinction could be made between light and dark individuals. The writer has observed both light and dark brindles in Greyhounds, but no records were available upon their breeding behavior.

The results upon white spotting in Greyhounds agree quite closely with those of Dunn, Webb and Schneider[2] upon Holstein cattle. In each instance the major factor is a recessive one

INHERITANCE OF SPOTTING

Figure 3

A litter resulting from the mating of a white-spotted female which was predominately white to a similar male. The offspring are practically all predominately white showing the recessiveness of the extreme degree of white on white spotted hounds.

for spotting and the degree of spotting is dependent upon genetic factors. In Greyhounds as in Holsteins the more extended white condition was more or less recessive to the pattern where the white area was more limited.

It appears that in the case of both Greyhounds and Great Danes, variations in coat color are due primarily to extension factors acting upon black. This is evidenced both by the fact that all individuals possess black muzzles and that no liver color is found in these breeds. Liver color is found in other breeds of dogs and is probably identical with chocolate which behaves as a recessive allelomorph to black in smaller mammals. If liver be an allelomorph of black and the breed be homozygous for black, no liver colored animals would be expected to appear.

Summary and Conclusions

1. Black, brindle, and fawn or red compose an allelomorphic series in Greyhounds.
2. The order of dominance is black to brindle and fawn or red, and brindle to fawn or red.
3. Fawn is probably the result of dilution factors acting upon red.
4. Blue is the dilute condition of black, but probably not due to the same factor as fawn.
5. White spotting is recessive to self color.
6. The extent of the white area in white spotted Greyhounds is dependent upon genetic factors, the more extended white condition being recessive to the more restricted.
7. White Greyhounds are genotypically white spotted individuals in which the white area has been extended to a maximum.
8. All colors in Greyhounds appear to be due to extension factors acting upon black, for which the breed seems to be homozygous.

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TABLE I—Results of Crossing Various Greyhound Colors

	Black	Black Spotted	Brindle	Brindle Spotted	Fawn	Fawn Spotted	Red	Red Spotted	Blue	Blue Spotted	White
Black x Black	39	29	15	0	4	1	1				
Black x Brindle	122	29	127	21	9	11	12	3	25	7	6
Black x Fawn	50	30	18	5	23	1	5	1	5	3	2
Black x Red	30	12	22	5	13	5	11	1	1	0	1
Black x Blue	7	2	2		2		1	1	2		
Black x White	26	19	10	9	2	4	3		6		6
Brindle x Brindle	5	6	159	34	26	9	20	4	2		4
Brindle x Fawn	4		114	28	50	10	33	7	1		1
Brindle x Red			100	8	25	6	15	4			1
Brindle x Blue	19	1	43	8	1		4		9	6	
Brindle x White	2	6	25	31	10	7	1	2	3		4
Fawn x Fawn				1	32	1	14				
Fawn x Red	1		8	2	43	7	46	2			
Fawn x Blue	19	5	17	1	18	2	1		5		1
Fawn x White	3		18	12	4	8	8	1			6
Red x Red	1	3	4		7	1	27	3			1
Red x White	1		9	12	3	6	4	1			
Red x Blue	4		3	1	4		2		4		
Blue x White	7	15	7	3	8						

TABLE II—The Results of Crossing Various Greyhound Colors. (Same as table I except that spotted individuals have been included along with the selfs of their color.)

MATING	BLACK	BRINDLE	FAUN	RED	BLUE	WHITE
BlackxBlack	68	15	5	1	-	-
BlackxBrindle	151	148	20	15	32	6
BlackxFaun	80	23	24	6	8	2
Black x Red	42	27	18	12	1	1
Black x Blue	9	2	2	2	2	-
Black x White	45	19	6	3	6	6
BrindlexBrindle	11	193	35	24	2	4
Brindle x Faun	4	142	60	40	1	1
Brindle x Red	-	108	31	19	-	1
Brindle x Blue	20	51	1	4	15	-
Brindlex White	8	56	17	3	3	4
Faun x Faun	-	1	33	14	-	-
Faun x Red	1	10	50	48	-	-
Faun x Blue	24	18	20	1	5	1
Faun x White	3	30	12	9	-	6
Red x Red	4	4	8	30	-	1
Red x Blue	4	4	4	2	4	-
Red x White	1	21	9	5	-	-
Blue x White	22	10	6	-	-	-

TABLE III—Comparing the Results upon Greyhounds with Those upon Great Danes.

Mating	Greyhounds			Great Danes		
	Black	Brindle	Faun	Black	Brindle	Faun
BlackxBlack	68	15	6	309	22	2
BlackxBrindle	183	148	35	36	37	16
BlackxFaun	88	23	30	13	16	4
BrindlexBrindle	13	193	59	9	492	73
BrindlexFaun	5	142	100	1	292	185
Faun x Faun	5	15	183		4	60

TABLE IV—Results upon White Spotting for Crosses of Black by Black and Brindle by Brindle.

	BLACKS			BRINDLES		
	Self	Spotted	White	Self	Spotted	White
Self x Self	59	30	0	212	53	4
Self x Spotted	68	81	7	168	128	4
SpottedxSpotted	6	67	1	9	54	7
Self x White	47	32	6	41	46	4
SpottedxWhite	6	33	8	4	37	?

TABLE V—Results upon White Combining Data from All Colors.

	Self	Spotted	White
Self x Self	1333	315	17
Self x Spotted	752	680	43
Spotted x Spotted	46	362	19
Self x White	158	137	15
Spotted x White	10	81	11

TABLE VI—Results upon Degree of Spotting from Data upon Blacks and Brindles.

MATING	SELF	PREDOM. SELF	PREDOM. WHITE	WHITE
Predom. Whitex Predom.White		7	33	4
Predom.Col.x Predom.Col.	12	27	18	3
Predom.Col.xPredom. White	3	25	11	1
Self x Predom. White	81	29	49	7
Self x Predom. Colored	155	83	48	4
White x Predom. White	6	3	20	4
White x Predom. Colored	4	11	36	6
White x Self	88	41	37	10

RECENT PROGRESS IN GENETICS

Summary of the Report of the Department of Genetics, Carnegie Institution of Washington

GENETICS is that branch of biology which is concerned with the phenomena of inheritance and variation. Among plants and animals offspring tend to resemble parents and ancestors but they never exactly resemble them. There are always differences. Genetics seeks to discover the laws governing such of these similarities and differences as are inheritable.

Rediscovery of Mendel's Law

The science as a distinct and recognized branch of knowledge is very young. Its history runs back no further than 1900 when a law upon which inheritance is based was rediscovered. Thirty-four years before, this law had been announced by Gregor Mendel, an obscure Czechoslovakian prelate, but it made no impression on the scientists of his day. Its significance was recognized, however, in 1900 when European botanists working independently demonstrated anew Mendel's law. The science of genetics begins with the rediscovery of this law.

Mendel, for example, crossed a tall variety of garden pea to a short variety. The offspring were all tall. These were allowed to self-fertilize naturally. Their offspring were tall and short in the ratio of three talls to one short. Mendel carried his experiments in crossing peas much further. His explanations of the numerical ratios which he obtained in successive crossings were based on the assumption which he made that certain units or elements convey the hereditary characters and that these pass from parent to offspring by way of the germ-cells. Recent research has proved that his assumption was correct. It has been discovered that the carriers of these character elements are the chromosomes which are present in all germinal matter.

Progress in the Field of Genetics

Research in this field has centered upon the behavior of these chromosomes. While Mendel's conclusions, so far as they went, have been verified, yet the process involved in inheritance is now seen to be very much more complicated than he thought it. It has been found for example that certain characters tend to remain together throughout subsequent generations. This tendency is called "linkage." Then again there are many cases in which the character elements break apart from their original combinations forming new combinations or "cross-overs." As a result of these discoveries Mendel's law as it now stands after a quarter of a century of research endeavor, though retaining all that Mendel formulated, is much more elaborate.

In describing the progress of Genetics Dr. Charles B. Davenport, in his 1927 report to the Carnegie Institution says of these matters:

> The studies of the first half of this period (since 1900) led to the recognition of the wide applicability of Mendel's laws, but revealed unconformable cases. The studies of the second half of this period brought many unconformable cases under the general laws of linkage and crossing-over on the one hand, and chromosomal aberrations on the other.

Sex-determination, in Sciara, a Genus of Flies

Dr. Davenport reports that studies on chromosomes by staff members have continued throughout the year resulting in the discovery of new deposits of rich material. In this connection he speaks in detail of investigations by several members of the staff. The work of Dr. C. W. Metz on a genus of flies called *Sciara* will

give some indication of the progress being made.

Dr. Metz has discovered certain species of this fly in which the offspring of pairs of flies uniformly give progeny which are all of the same sex or nearly so, whereas in other species the ordinary procedure prevails in which each female produces equal numbers of both male-producing and female-producing eggs. To quote from the report:

> The details are full of interest. When individual females are mated to one male after the other, the progeny are all of the same sex, regardless of the number of males used. When one and the same male is mated to two or more females the total of his offspring may include large numbers of each sex; while one of the females often has only daughters and the other only sons. The conclusion is that the females are immediately responsible for the sex of the offspring.
>
> It accordingly appears that at fertilization the eggs of the male-producing females permit the sex-limited chromosome brought in by the sperm to remain in the fertilized egg. On the contrary, the eggs of the female-producing females presumably exclude the two large sex-limited chromosomes that are brought in by the sperm or extrude those sex-limited chromosomes later. In species that have the ordinary bisexual progeny, presumably each female produces equal numbers of the two types of eggs.
>
> We do not yet know whether sex is influenced directly by the sex-limited chromosome or is primarily dependent on other chromosomes. Part of the above-described process is still hypothetical, as not all of its details have been observed. The mechanism especially by which the two sex-limited chromosomes are excluded from some eggs and not from others remains to be worked out. Enough has been learned, however, to make it clear that, while in other flies the sex is determined by a difference in the chromosomes of the two kinds of sperm in *Sciara* (the sperms being alike) the determination of sex is a function of the female egg.

Dr. Metz concludes from evidence which he has obtained that in the ancestors of *Sciara* the males produced the two kinds of sperm and were responsible for "sex determination," as in most other known groups of flies. In this genus, however, on account of the loss of one of the two kinds of sperms, the function of sex determination has apparently been forced upon the egg, or else assumed by it. It seems possible, the report adds, that in this study of *Sciara* something may be learned of the evolutionary process by which the change from sperm determination to egg determination of sex has been brought about.

The Phenomena of Growth

Thirty-five years ago biology was greatly occupied with the processes by which an organism increases in size, ultimately assuming a definite form. Dr. Davenport points out that while the spiral movement of science has brought biology again face to face with the mechanics of development it now views the matter from the new standpoint of knowledge that the chromosomes in some unknown way direct development from the inside. He declares that this control is a biochemical process of refined and complicated character.

Two phenomena relating to this matter of development, Dr. Davenport states, have been found particularly suitable for study for it has seemed possible to control and alter them in the laboratory, thereby giving an insight into the mechanism involved in their production. One of these is growth, the other is sex, and to both of the phenomena members of the laboratory staff have given especial attention.

Growth in Mice Embryos

In this study Dr. E. C. MacDowell in cooperation with Dr. Ezra Allen, dissected 1151 mouse embryos from their enveloping tissues and weighed them on scales having a delicacy within one one-hundredth of a milligram. 192 of these embryos ranged in age from 5 to 7½ days, dating from the fertilization of the egg; while 959 ranged from 8 to 18 days. This careful study was made in order to determine if possible, the true curve of prenatal growth.

It was found that the first seven days of embryonic life are consumed

in the work of implanation in the body tissues. Not until seven days have passed does the group of cells out of which the embryo proper is to arise begin to develop. This is thought to be the zero point of development. From that time forward until birth, the study shows, growth is enormously rapid. So regular, however, is this increase that the average weight for successive days, plotted on logarithmic paper, approximates a straight line.

MacDowell has shown that this relation to a straight line is not accidental by comparing his data from mice with the data of other investigators working with guinea-pigs. He has also checked them with the growth of chick embryos. Thus it appears that the prenatal growths of these three animals advance at essentially the same relative rate.

Attempts to Alter Course of Growth

Other studies on mice were made to determine the extent to which the course of growth could be changed by making alterations in the thyroid gland of the mothers before gestation began and by removing the reproductive organs of the young ten days after birth.

The first of these experiments was undertaken partly to see if some light would be thrown on the production of human dwarfs. Many have a history of severe emotional or physical strain in the mother. The possibility has been suggested that the disturbance of the thyroid secretions due to such stress may be, at least partly, responsibile for dwarfs.

This experiment with mice showed that those born of mothers in which the gland had been completely removed developed faster than their controls; while those born of mothers in which the gland had been merely singed grew more slowly than their controls.

In respect to the young in which the reproductive organs had been removed it was found that despite the severity of the operation the growth was only slightly retarded. It seems, therefore, that such changes as occur in growth rates after birth are independent of secretions from the reproductive organs.

Can Sex in Mammals be Changed at Will?

Reparding the study of problems relating to sex, Dr. Davenport says:

> It is a statistical fact long known, that the proportion of sexes in man and mammals deviates somewhat from the expected proportions of fifty to fifty. The causes responsible for this deviation and the control of the proportions remain unknown. We have seen that in the fly *Sciara*, the method has been worked out by which the sex ratio is under the control of the mother or, more specifically, of her eggs. It is believed that in birds and mammals the proportion of the sexes can be disturbed by certain procedures.
>
> For example, various attempts have been made to influence the sex ratio through the differential susceptibility of the two kinds of sperm to different treatments. Some investigators profess to have secured results from the alcohol treatment of male mice. Since the results are open to the criticism of being based on insufficient numbers and of being insufficiently controlled, MacDowell has undertaken the re-investigation of the subject.
>
> Male mice were subjected to the heaviest alcohol treatment that they could survive, five times a week by the inhalation method. These mice were mated with normal females whose successive litters were sired, alternately by a treated male and his untreated litter mate. The control of all possible influences besides the alcohol treatment was carried out in great detail. The deviations of the ratios from the fifty-fifty proportion were found to be less than the probable errors. The conclusion therefore that sex ratio in mammals can be altered by the presence of alcohol in the parental blood has thus been overthrown by the critical work of MacDowell.

Dr. Davenport sums up the results of this and other experiments designed to determine the cause of differences in sex ratios by saying that it has not yet been shown that conditions external to the fetus determine the difference in the proportion of the two sexes. He adds: "Clearly the sex-

differential in the protoplasm is chiefly located in the chromosomes."

Crustaceans Adapt Themselves to Living in Hot Water

Among a number of studies described by Dr. Davenport relating to the genetics of special traits that by Dr. A. M. Banta and his associates on *Daphnia,* a genus of minute fresh water crustaceans, is of especial interest.

A year ago, during a period of hot weather, a female was hatched which produced several broods of young. The broods hatched during a low temperature died; those which hatched during hot weather survived. This suggested to the observer that perhaps this *Daphnid* required water at a higher temperature than that in which *Daphnids* usually thrive, which is about 68° F. So mother and daughters were placed in a temperature of 80°, which is usually fatal. They thrived. Further experiments showed that they were vigorous at a temperature between 77° and 86°, and that they did not die until a temperature of 110° had been reached. The parent stock, on the other hand, succumbs at 100°.

The particular interest of this experiment lies in the origin of races of animal life that in nature thrive in hot water. Instead of being the produce of long acclimatization they may be the result of variations that arise very suddenly. Survival at high temperatures might be followed by yet other sharp variations, enabling the descendants successively to live at higher and higher temperatures until adaptation to abnormal temperatures were finally obtained. Banta's studies show, the report adds, that a sudden variation in a given direction may be followed by further variations in the same direction, the effects thus becoming cumulative.

Comparative Study of Mental Traits of Jamaican Natives

In connection with investigations in the field of human genetics, Dr. Davenport describes a number of interesting studies.

Under a special fund provided by Colonel W. P. Draper, a comparative research is being made into the physical and mental traits of full-blooded negroes, whites and mulattoes in Jamaica.

The work consists of two main parts—the comparative physical and mental measurements of lots of fifty persons of each sex, who are as nearly as possible living under the same conditions, who are of about the same social stratum, and who represent respectively the white race, full-blooded negroes, and hybrids. Besides mental and physical measurements, several investigations into physiology are being made, such as oxidation of the blood, blood grouping and, with the assistance of Dr. F. G. Benedict of the Nutrition Laboratory, a study of basal metabolism. Dr. B. E. Washburn of the Rockefeller Foundation is also collaborating. Data on a number of white families which have lived for several generations on the island of Seafordtown, Jamaica, have also been obtained and are being tabulated.

Heredity of Goiter

Finally, Dr. Davenport records the conclusions reached in a study of the conditions under which goiter has arisen in 104 families in Maryland. He says:

> In collaboration with the Playground Athletic League and with the assistance of Dr. L. A. Nelson, a study has been made of the conditions under which goiter has arisen in 104 families of Maryland. As is well known goiter affects particularly the female. When both father and mother are goiterous all of the daughters (5), who have reached the age of 14 years are goiterous.
>
> In another case where the father alone is goiterous, though the mother has goiterous relatives, the daughters (2) are both goiterous. Where the father alone is goiterous, and there is no goiter on the mother's side, the daughters (3) are all goiterous. When the mother alone is goiterous, but the father has goiterous relatives, then about 90 per cent of the grown daughters are goiterous (40 cases).

Even when the mother is affected and there is no evidence of goiter on the father's side the proportion of grown daughters who are goiterous is about 80 per cent. On the other hand, where neither father nor mother is goiterous the proportion of affected grown daughters is reduced to 60 per cent or less, even in those families which were selected because of goiterous examples in the ancestral line.

ORIGIN OF THE FLORENTINE BIZZARRIA

Castello—near Florence, Italy.
May 10, 1927.

Mr. Charles F. Swingle,
Bureau of Plant Industry,
Washington.

DEAR SIR:

I have read with much interest your important note on graft hybrids, published recently in the JOURNAL OF HEREDITY. Allow me, pray, to send you some notes concerning the Florentine or Panciatichi Bizzarria, about which much has been incorrectly written.

It is not true that this Bizzarria has been originated with any artifice. Pietro Nati, in his work says that it was produced "*nulla malorum adaptione; nullo factionis artificio, nulloque adulterii ingenio; sed solo eventu genioque naturae. . .*"*— Its origin was from a natural decapitation of an old tree of the Florentine Citron, dead from an unknown cause. This species of Citrus was largely grown in the Florentine gardens in the oldest times, both in large pots (brought into orangeries in autumn) and outdoors, in sheltered positions against walls or house fronts in the form or little groves (boschetti) that were protected during winter with some covering of wood or straw mats.

It is necessary to add that these plants were always grafted on sour orange stock (*Citrus aurantium amara*) because it was the most vigorous of all the species of Citrus. Professor Nati says: *quippe ceteris congeneribus vegetiores.*†

Giuseppe Piccioli, head gardener botanist of the Grand Duke of Tuscany at the Museum of Florence, and who had been head gardener at Villa Panciatichi about the year 1780, author of the *Hortus Panciaticus* (therefore in position to be well informed about the origin of the Bizzarria, although writing in a later time) in a little book on flower cultivation, in pointing to this plant writes:

"Ma avendo egli—il giardiniere di Casa Panciatichi—un boschetto di Cedrati ed essendogliene andata a male una pianta, o per il freddo o per qualche altro motivo, detta pianta repullulo dal piede sobra il selvatico, dove essendo cresciuta per negligenza del giardiniere non fu innestata, ma fu lasciata crescere e cominciando a fiorire allego i suoi frutti i quali venuti alla debita maturità furono esaminati e ritrovati essere di due specie, cioè che in un solo frutto lo vedeva il Cedrato e l'arancio". (See: Piccioli—La Cultura dei fiori, etc., 4ª Ed. Firenze—presso Anton Giuseppe Pagani e Comp.—1792—page 31).‡

Therefore it is evident that the Bizzarria had its origin from the trunk of

*By no modification of the fruits; by no ingenuity in handling; by no skill in crossing; but solely as an act of nature.

†For it is more vigorous than other kinds.

‡But since the gardener of Panciatichi House had a little grove of citrons, and since one plant had gone bad on him, either because of cold or for some other reason, this plant pushed out from the stem above the stock where it grew. And because of the negligence of the gardener, it was not grafted but was allowed to grow and soon began to flower. Its fruits, having come to proper maturity, were examined and found to be of two species. such that in a single fruit one saw citron and orange.

an old tree of Citrus, which had supported for many years the branches of a Florentine Citron.

The opinion of Strasburger that the Bizzarria may be a sexual hybrid, cannot stand without demonstrating the possibility of obtaining such a cross; because, if the information of Piccioli is exact it would be necessary to admit that the graft of the Florentine Citron was originally made on this hybrid stock. But neither myself nor anyone else has ever been able to obtain this cross.

It must be noted, however, that Professor Nati, the first illustrator of this plant had the exact perception of its nature when writing: "In hoc multiplici, et longa ennorum serie iterato arborum conjugio contingere aliquando potuisse, ut ex confinio ocularis insitionis, in quo temporis diuturnitati permixta utriusque aboris natura concreverat; quam varii per communes fibrarum ductus permeantes humores diu aluerunt; *unum aliquando germen utramque malorum speciem probe reservens amerserit. . .*"§

I have never seen Piccioli's testimony cited by the men of science who have treated with the Bizzarria. If you will make it known by means of the JOURNAL OF HEREDITY, it would give, I think, a good indication of the true history of this plant.

Allow me also to note a *lapsus calami* which occurred in your article—Lucius Junius Moderatus Columella, born at Cadiz 47 A. D., wrote his books at Rome, at the time of Emperor Claudius. Plinius mentions him in his history.

Yours sincerely,

DR. RAGIONIERI.

Analysis of Racial Groups

WALTER SCHEIDT, RASSENFORSCHUNG. EINE EINFÜHRUNG IN RASSENKUNDLICHE METHODEN. Pp. 82, 5.80 marks. George Thieme, Leipzig, 1927.

The author attempts to give in his pamphlet an outline of the methods usually employed in the racial analysis of population groups. Three questions determined the arrangement of his material: 1 how shall the data be chosen?, 2 how shall they be analysed?, and 3 how shall they be presented? In answering these questions, Scheidt gives a clear and concise sketch of this field of anthropological investigation. In a short pamphlet, however, he cannot give more than a sketch, and one must ask to whom such a picture is of real value? The layman always will be interested in results rather than in methods, and the student, even the beginning student, will have to acquaint himself with the problems in question in much more detailed and critical way, even if he only wants to follow the literature intelligently. *W. L.*

§In this frequent and repeated union of the trees during a long period of years it could have happened sometime, that from the border of a graft where the substance of the two trees had been mixed and grown for a long time together and were nourished by the different fluids flowing through the common vessels of the fibres, one had at some time appeared, retaining plainly the characteristics of both fruits.

ELIMINATING RECESSIVE DEFECTS

October 26th, 1927.

Editor, JOURNAL OF HEREDITY:

DEAR SIR:

In the interesting and instructive article by Professor H. S. Jennings on Health and Race Progress in your issue of June last, he refers to Professor Punnett's articles in your Journal of 1917 (VIII, page 464) as indicating that to cancel those persons who are definitely feebleminded would make "almost no progress towards getting rid of feeblemindedness for later generations."

Perhaps you will allow me to call the attention of your readers to Dr. R. A. Fisher's comments on Professor Punnett's paper which were published in the *Eugenics Review* in 1924 (Vol. XVI, page 114). The calculations given by Professor Punnett are no doubt accurate and go to prove that, on his assumptions, it would take ninety generations (say 2,700 years), to reduce the proportion of defectives from 1 in 100 to 1 in 10,000. This sounds very discouraging, but Dr. Fisher points out that on exactly the same basis it can be shown that the proportion would be reduced by over 17 per cent in the first generation, this being a gain surely worth striving for. Moreover, Professor Punnett assumes that no assortive mating takes place, which is an erroneous assumption.

Taking this fact also into consideration, it may well be that by suitable measures the proportion of the feebleminded might be reduced by 36 per cent or more in a single generation. The results to be obtained would, of course, depend on the degree of feeblemindedness held to be a bar to parenthood.

Yours faithfully,
Leonard Darwin,
President.

Since Professor Punnett's original calculations were published in the JOURNAL OF HEREDITY, it seems desirable that Dr. Fisher's rearrangement of these figures should also be available to our readers. We are therefore reprinting Dr. Fisher's article, which originally appeared in the *Eugenics Review*.—Editor.

The Elimination of Mental Defect

R. A. FISHER

Vol. XVI, No. 2, July, 1922. Pp. 114-16.

IT has been repeatedly brought to my notice in recent months that some calculations published by Professor Punnett seven years ago (JOURNAL OF HEREDITY VIII, 465) have led to a widespread misapprehension of the effectiveness of selection, either by segregation or sterilization, in purging a population of its feebleminded strains.

The table in which Professor Punnett has inadvertently supplied material for anti-eugenic propaganda is the following, showing the number of generations required, by segregation or sterilization of recessives only, to decrease the proportion of defectives in the manner shown:

From 1 in 100	to 1 in 1,000	.. 22 generations.
From 1 in 1,000	to 1 in 10,000	.. 68 generations.
From 1 in 10,000	to 1 in 100,000	.. 216 generations.
From 1 in 100,000	to 1 in 1,000,000	.. 634 generations.

The figures are really alarming; most readers, after a hasty glance, would conclude that such elimination would be too hopelessly slow to be worth attempting. This impression is heightened by the increasing length of each

step, and could be further emphasized in the same way by carrying the table up to say 1,000,000,000—only at this point the reader would perhaps see the catch, and recognize that it would not matter if it took 10,000 generations to rid the world of its last lone feeble-minded individual!

The assumptions on which the table is based may be criticized, but let us first rectify the method of presentation, so as to show *on the same assumptions,* what would be the effect of such selection in our immediate posterity; expressing the frequency of defectives as so many per 10,000, we have:

From 100 to 82.6 in 1 generation.
From 82.6 to 69.4 in 1 generation.
From 69.4 to 59.2 in 1 generation.

This is as far as we need go to express the effect which one individual might live to experience. In a single generation the load of public expenditure and personal misery caused by feeblemindedness, if this is the particular defect considered, would be reduced by over 17 per cent. If our starting point had been 30 instead of 100, still a single generation of selection would lighten the burden by over 11 per cent. Even on the assumptions underlying Professor Punnett's figures, it is within our power to leave our children a substantial eugenic legacy. This legacy would be permanent, even if our children decided not to diminish the proportion of feebleminded still further; it would, however, be in their power, merely by continuing the same administrative measures, nearly to double it in handing it on to the third generation; further, if any advances were made in genetical knowledge, or administrative wisdom, they could add more substantially than we can to the accumulated capital of social fitness.

Turning now to the assumptions upon which the above calculations are based, we find that they are two.

(1) That feeblemindedness may be equated to a mendelian recessive, (2) that the population chooses its mates *at random.* Both assumptions, especially the second, are unfavorable to the effectiveness of selection.

Let us consider the first assumption. There is a considerable body of pedigree evidence indicating the existence of a single mendelian factor which, in its recessive phase, is capable alone of producing feebleness of mind. There is an even more substantial body of evidence that normal people, as well as the feeble in mind, differ very greatly in degree in just the same mental characteristics as distinguish the feeble-minded from the normal. Consequently, while we may speak of feeblemindedness as due to a mendelian recessive, no responsible authority would claim that all feebleminded cases are genetically alike. There are doubtless many mendelian factors of sufficient importance to decide the fate of borderline cases; possibly there are several factors each capable alone of producing feeblemindedness; the latter, possibility is paralleled by the case of stationary night blindness, which it is known may be caused by either an ordinary dominant factor, or by a sex linked recessive. It is possible to distinguish these two factors owing to their marked difference in inheritance; had they been inherited in the same way, no one would have thought of regarding them as genetically distinct. Consequently, in the case of so variable a characteristic as feebleness of mind it would be extremely rash to assume that only one main factor is present, and entirely contrary to the evidence to ignore the contributions of less important factors.

The second assumpion is open to even more obvious criticism. Mating is very largely controlled by social class, and the feebleminded undoubtedly gravitate to the lowest social stratum. Further, within each class there is a decided tendency for like to mate with like. In stature the resemblance of husband and wife is nearly as great as that of uncle and niece; it can scarcely be doubted that mental similarity is equally, or more, effective;

those who find their "affinity" among the feebleminded, are surely not to be taken as average citizens.

It is difficult, for lack of data, to take accurate account of these facts. If instead of regarding the feebleminded as 1 in 320 taken at random in the general population, we were to regard them as constituting one-sixteenth of an intermating group constituting 5 per cent of the general population, then the effect of segregation would be to reduce the incidence of the defect by 36 per cent in one generation. This is perhaps about what might be expected from an effective policy of segregation, and it is of a magnitude which no one with a care for his country's future can afford to ignore.

There is some direct evidence that the above is not an over-estimate. If the factor for feeblemindedness were really scattered at random in the population as Punnett assumes, then among the parents of the feebleminded only 5.5% would themselves be feebleminded. If the factor were concentrated to the extent assumed above, 25% of the parents would be feebleminded. I know of no satisfactory published data on this point, but Major Darwin informs me that the parents of 325 feebleminded cases given by Goddard may be analyzed as follows:

39%	Normal	including several cases stated to be alcoholic,
2%	Normal (?)	sexually immoral, tuberculous, migrainous, or neurotic.
14.6%	Not reputed normal nor diagnosed as feebleminded, but stated to be either alcoholic, criminalistic, epileptic, insane, migrainous, neurotic, paralytic, tuberculous, or tramps.	
14.9%	Facts unknown.	
29.5%	Feebleminded.	
100.0		

The lowest possible estimate of the percentage of feebleminded deriveable from these figures is 29.5%, and indicates a distinctly higher degree of concentration of the defect than I have assumed. Actually the fairest estimate which we can make is probably that based on the definitely feebleminded and the definitely normal only; of these 43.1% are feebleminded. It is, however, sufficient for the immediate question that the proportion of the parents of the feebleminded who are themselves feebleminded is about 25—50% and far greater than the 5.5% of Punnett's suppositions; and that the social benefit of segregation, or sterilization, is correspondingly greater even than should be inferred on his assumptions.

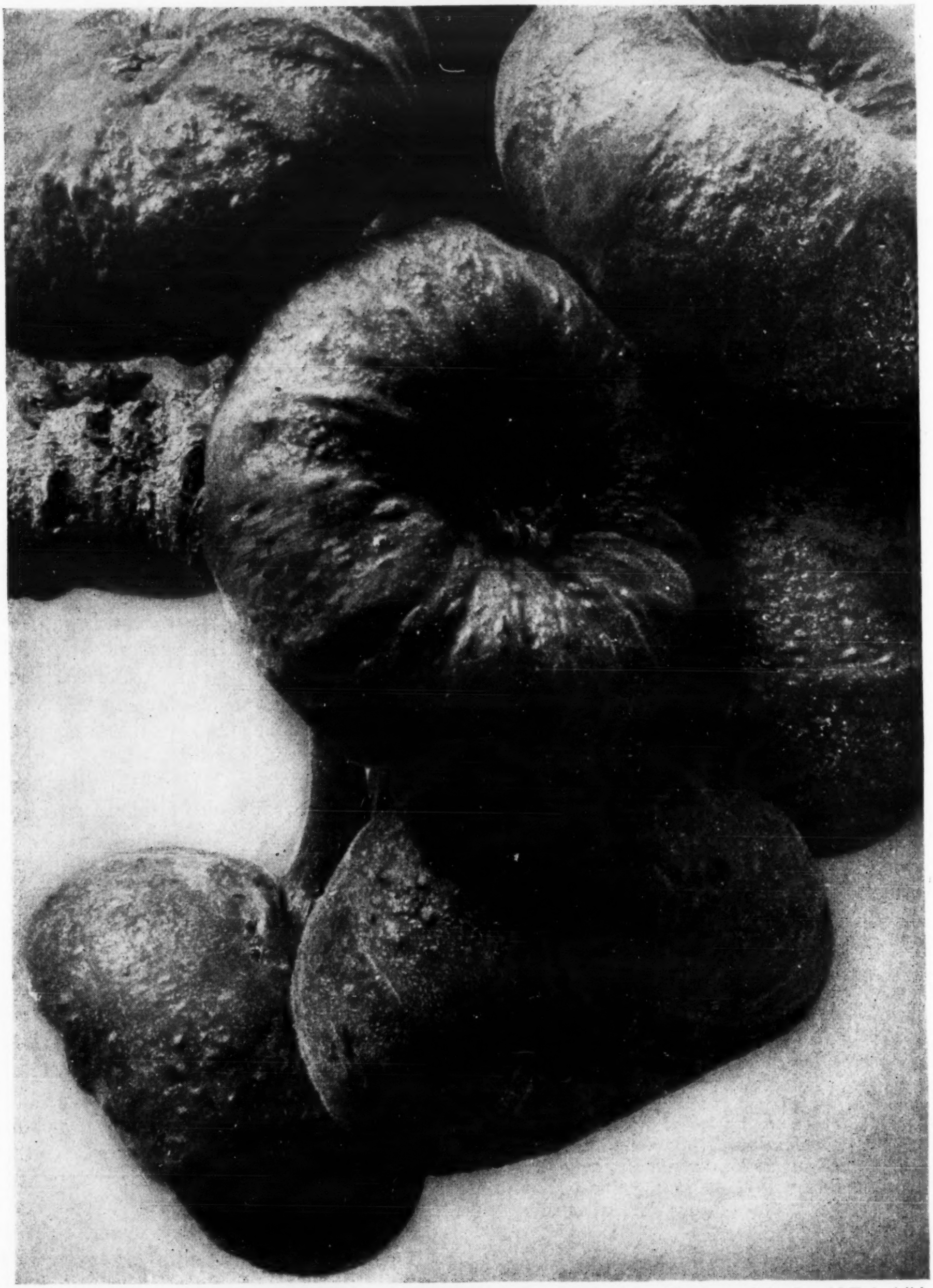
It is true that in the absence of further advances in general knowledge, the *rate* of racial purification must be expected to fall off. However, in the question of mental defect, it is extremely improbable that improved methods of detailed diagnosis will not be followed by a further discrimination of the genetical types present. Indeed it was the main point of Professor Punnett's paper, although a point ignored by those who use it only for anti-eugenic propaganda, that further study might make it possible to diagnose the normal carriers of the factor for feeblemindedness, and in this way greatly to accelerate the speed with which the defect could be eliminated.

We may therefore sum up in the following three conclusions:

(1) Even on the most unfavorable assumptions the segregation or sterilization of the feebleminded would lead to substantial immediate progress in the elimination of the defect.

(2) The concentration of the defect in a limited number of strains, to the extent which appears to be the case, renders the probable rate of progress as great as any to be expected in the field of diseases capable of remedial control.

(3) There is no reason to anticipate that the rate of progress will fall off in subsequent generations, provided that genetical research is as successful in the future as it has been in the present generation.



Photograph by David Fairchild

THE LARGEST FIG

Figure 4

A bunch of Roxburgh figs (*Ficus Roxburghii*) from the tree in the Botanic Garden in Teneriffe. The giant fruits are of a woody consistency until the ripening process is initiated by stimulating the female flowers mechanically, whereupon the fruits swell to a much larger size and turn soft. Natural Size.

ROXBURGH'S GIANT FIG IN TENERIFFE*

DAVID FAIRCHILD

I HAD heard before of Roxburgh's fig (*Ficus roxburghii*). In fact my friend Dorsett and I once photographed the fruits of one which we found growing on the place of Mr. Coppinger at what was called the "Alligator Farm" near Miami. It was a small tree and the fruits were borne near the ground in a curious fashion. It was said to have been bronght from "up the State" from the old place of that pioneer horticulturist, Wilder.

But nevertheless I was utterly unprepared for the amazing sight of a full grown tree of this giant-fruited fig which I saw in the Botanic Gardens of Orotava, Teneriffe.

It seemed quite the most amazing example I had ever seen of what is called a cauliflorous tree. Even the famous cannonball tree (*Couroupita guianensis*) is scarcely more grotesque than Roxburgh's fig tree in this respect.

I walked under the old specimen and everywhere that my eyes turned I saw figs. Not ordinary-sized figs but giant figs. They were not borne singly as are the ordinary fig but in great clusters. On the small branches they hung. The crotches of the larger branches were filled with them. There were piles of them growing from the base of the large trunk. The whole tree seemed a mass of giant figs.

These fruits were great, brown, woody-looking things with small brown dots over their surfaces. I measured them and found many which were three inches across, and an inch thick. None seemed to be ripe.

While I was in the midst of my examination of them Don Juan Bolinaga, the Director of the Garden, appeared. He was evidently very proud of this Himalayan fig and the way it grew in his garden, and he might well be, for it is so rare a species that I failed to find it in the collections in Ceylon, Sumatra or Java. He had tried to propagate it by the various methods which he knew. He insisted that although he had been successful, it required two year's time for him to get well-rooted branches of it. His process was that of binding a branch with moss and keeping the moss wet constantly. All other methods had failed.

When I enquired if the fruits were good to eat he declared that they were when properly stimulated by hand. By means of a stick he irritated through the ostiolum the stigmas of the female flowers which lie behind it. He declared that then the fruit would swell up until it was as large as a saucer, turn soft, and be good to eat. He recounted how nine people had partaken of one fig. He did not maintain that they were people passionately fond of figs,—I am sure that my son, for example, could have taken the place of the nine people. But it conveyed, as perhaps no other story could, the idea of size. It is an amazing fig.

When I translated the account which Don Juan Bolinaga, the Director, had given me to the Utowana's company they all demanded that he tickle the ostiolum of one or two fruits and give a demonstration. Since we were going to be in the island several days he thought he might show us one of the ripened figs before we left.

But it appeared to be too early in the season for the figs to respond to the stimulus of the irritation and none of them swelled as Signor Bolinaga had hoped they might. In November, however, three months later, I got a letter from Don Juan in which he enclosed an outline, the correctness of which I do not doubt, was *four inches* across. A large Smyrna fig would be perhaps

*Plant Introduction Contribution of the Allison V. Armour Expedition, 1925-1926.

Photograph by P. H. Dorsett

FIGS BORNE ON THE ROOTS

Figure 5

Roxburgh figs borne at the base of a young tree standing on the Alligator Farm near Miami. These fruits are not simply lying on the ground—they are attached to the large roots of the tree. The production of fruit on the roots, and from fruit spurs arising anywhere on the trunk or large branches is a striking character of the species.

Photograph by P. H. Dorsett

ROXBURGH'S FIG IN FLORIDA

Figure 6

A young tree of the immense leaved Roxburgh's fig (*Ficus Roxburghii*) on the grounds of the so-called Alligator Farm at Miami, Florida. The tree was sent to the owner of the farm, Mr. Coppinger, from somewhere in the northern part of the state. It is a Himalyan species and able to stand cool but probably not freezing weather.

two inches in diameter before it is dried and flattened out in a box, so that Roxburgh's fig, which is twice as large in diameter, produces the impression on one of having a mass represented by the cube of two, or eight. It has, in fact, eight times the volume of a large Smyrna fig.

I have not been able to discover in the floras of British India any but the most casual references to this fig so far as its edible qualities are concerned. This is not in the least surprising since few floras pay any attention to the economic values of the species which they describe. However, had it been of a quality comparable to the Smyrna fig the fact would certainly have been well known.

I am not sufficiently familiar with the facts to discuss the possibility of this species being hybridized with *Ficus carica*, the ordinary fig of commerce. I was surprised to hear my old friend Dr. Trabut declare in one of his lectures to the students at Maison Carrée, Algiers, that in his opinion *Ficus carica* itself was a hybrid and that its wild form had never been discovered.

If this should be true, then there might be a possibility of crossing this *Ficus roxburghii* with it and getting something quite different in size and in other characters from the ordinary fig.

But aside from any possible use as breeding material this tree deserves a place in South Florida and all regions with mild frostless winters because

of its broad spreading crown, attractive foliage and bizarre fruit formation.

Since the above was written on the S. S. Kirdorf en route to Ceylon, December 3, 1925, I have visited the tropical Botanical Gardens of Peradeniya, Ceylon; Sibolangit, Sumatra; Penang, Singapore and Buitenzorg, Java; and although I enquired for it at each one of these gardens I failed to find it. To my astonishment, however, I found it in the large greenhouse of Professor Goebel's Botanic Garden in Munich. From which I judge that it is not adapted to tropical conditions but is a truly Himalayan species to study which one must visit the Himalayan region of British India.

In a most interesting memoir by Dr. D. D. Cunningham, Surgeon General of the Bengal Army, entitled "On the Phenomena of Fertilization in Ficus Roxburgii, Wall.,* will be found a most detailed and masterful account of this species and the phenomena of its fertilization. The following statement, p. 45, from this memoir will serve to indicate how complicated the question of fertilization is in this dioecious species of fig, where, according to Cunningham, neither male nor female flowers develop without the stimulation which the fig insect affords when it enters the young male or female receptacles.

There can be little room for doubt that the phenomena indicate that, while the development of embryos in the female receptacles of the tree is essentially connected with the access of the insects to the receptacular cavity, it is yet normally independent of the introduction of pollen by their agency. The fact that the access of a single insect or a pair of them only is sufficient to determine the development of ten or twelve thousand embryos, is in itself almost conclusive against the occurrence of any ordinary process of pollination. The obstacles through which a passage has to be forced ere the receptacular cavity is reached are of such a nature and amount as to render it almost inconceivable that pollen should be introduced in sufficient quantity, and there is at the same time an absolute want of evidence to show that such introduction takes place. I have carefully examined very many receptacles at various periods shortly after access of insects to cavities, and have never been able to detect any evidence of general distribution of pollen over the stigmatic surface. Examination of individual flowers has given like results; in most cases it has been impossible to find any pollen within the receptacle or cavity, and in the few cases in which any was found it was represented by one or two shrivelled grains adherent to the corpses of insects.

It must be borne in mind, too, that if we accept the hypothesis that the development of the embryo is due to ordinary processes of pollination, we must assume not only that a single insect can convey many thousands of pollen-grains with it in spite of the excessive obstructions to access presented by the ostiolar plug, but that these grains are also methodically and economically distributed, for, unless each stigma were only allowed to appropriate a single grain, the amount introduced would have to be indefinitely multiplied.

The Law of Recapitulation

ONTOGENIE UND PHYLOGENIE: Das sogennante Biogenetische Grundgesetz und die biometabolischen Modi, *by* V. FRANZ. Pp. 51, price M.4. (Abhandlungen zur Theorie der organischen Entwicklung, Heft III.) Berlin, Verlag von Julius Springer, 1927.

Professor Franz of Jena defends the thesis that recapitulation deals with the embryonic, not the adult, characters of ancestral species, and it may take one of four forms (the biometabolic modi): prolongation, abbreviation, ontogenetically increasing, and ontogenetically culminating deviation. *P. P.*

*Published in the *Annals of the Royal Botanic Gardens,* Calcutta, Vol. 1, Appendix 1888. pp. 11 to 51, pl. I to VA.

A PISTILLATE PRUNUS*

CLAYTON O. SMITH
Riverside, California

BRANCHING HABITS OF HYBRID
Figure 7

At left, normal seedling of *Prunus umbellata* with characteristic branching; at right, *P. umbellata* hybrid, branches more nearly horizontal.

THE very unusual flowering habit of some seedlings of *Prunus umbellata* Elliot, which has been observed recently, seems worthy of record. About twenty of these seedlings have been under observation and seem to be fairly uniform in their characteristics. The foliage (Figure 8) resembles that of the mother tree, but often shows some differences. The leaves are larger and usually of a lighter green color; the base is broader; the tip is acuminate rather than acute, and the serrations differ somewhat in form. The branches (Figure 7) form a much greater angle with the stem, often being at right angles instead of at an angle of about 45 degrees as in normal seedlings of *P. umbellata*. The variation from the normal type is, however, most noticeable in the flowers. (Figures 10 and 9). The petals are much reduced in size, being little larger than the calyx lobes, by which they are concealed so that the flowers appear to be apetalous. In some seedlings these petals are larger than in others, but even here they are much

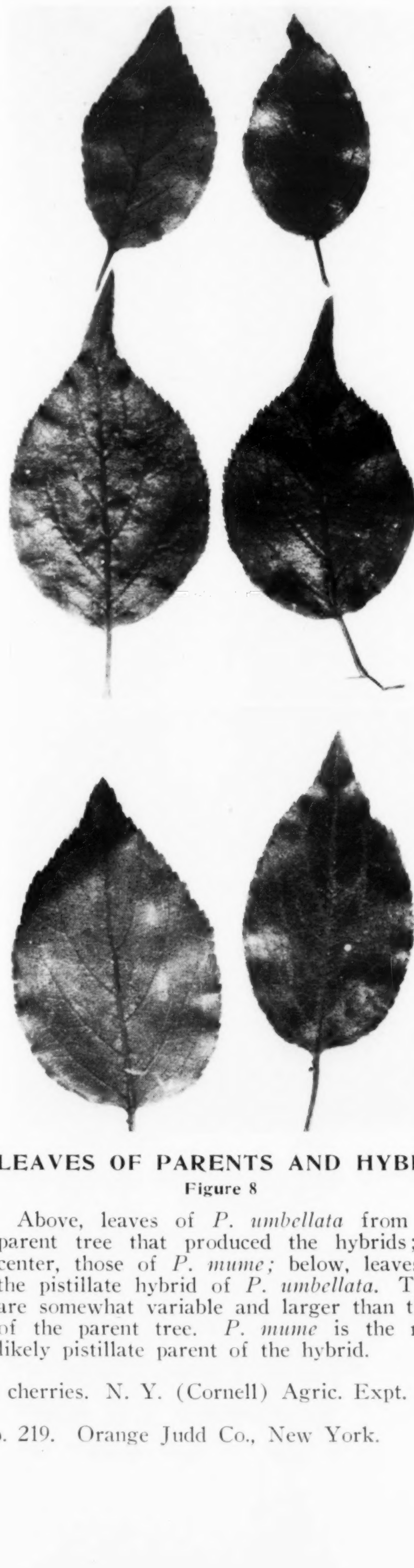
*Paper No. 169, University of California, Graduate School of Tropical Agriculture and Citrus Experiment Station, Riverside, California.

reduced. The calyx lobes have been modified from those of the species in color, shape, size and pubescence. They are brown instead of green, are much broader with a rounded tip, and have a smooth inner surface instead of being tomentose. Their margins are slightly ciliated. The flowers are without stamens, these apparently being replaced with from 10 to 20 tomentose pistils which are attached to the calyx cup in the same manner as are the stamens in Prunus. The fact that the supernumerary pistils are not attached separately to the receptacle but occupy the position of normal stamens in Prunus, indicates that they are to be considered as transformed stamens, and not as multiple or replicated pistils of the sort occurring in the strawberry. Apparently only one of them is attached to the receptacle.

Many flowers on five of these seedlings were artificially pollinated with pollen from several (at least four) different species of Prunus. Some of the flowers pollinated with pollen from the mother tree grew for some time and gave some promise of setting seed, but fell long before reaching maturity. Probably none of them were fecundated, and it may be that they were sterile. Observations were made on these small fruits. All but a single pistil in each flower soon fell off, with the calyx; the remaining pistil was tomentose and apparently plum-like in appearance, but it reached a diameter of only three millimeters before falling.

One seedling among these twenty had flower buds that fell off before they opened, corresponding in this characteristic with the two classical examples of plum-peach hybrids known as the Blackman* and Mule†, but differing from them in being pistillate instead of being staminate with deformed anthers.

The mother tree has the characteristics given for *P. umbellata* and seems



LEAVES OF PARENTS AND HYBRID

Figure 8

Above, leaves of *P. umbellata* from the parent tree that produced the hybrids; In center, those of *P. mume;* below, leaves of the pistillate hybrid of *P. umbellata.* These are somewhat variable and larger than those of the parent tree. *P. mume* is the most likely pistillate parent of the hybrid.

*Bailey, L. H. Cultivated native plums and cherries. N. Y. (Cornell) Agric. Expt. Sta. Bull. 38, p. 40.

†Waugh, F. A. Plums and plum culture. p. 219. Orange Judd Co., New York.

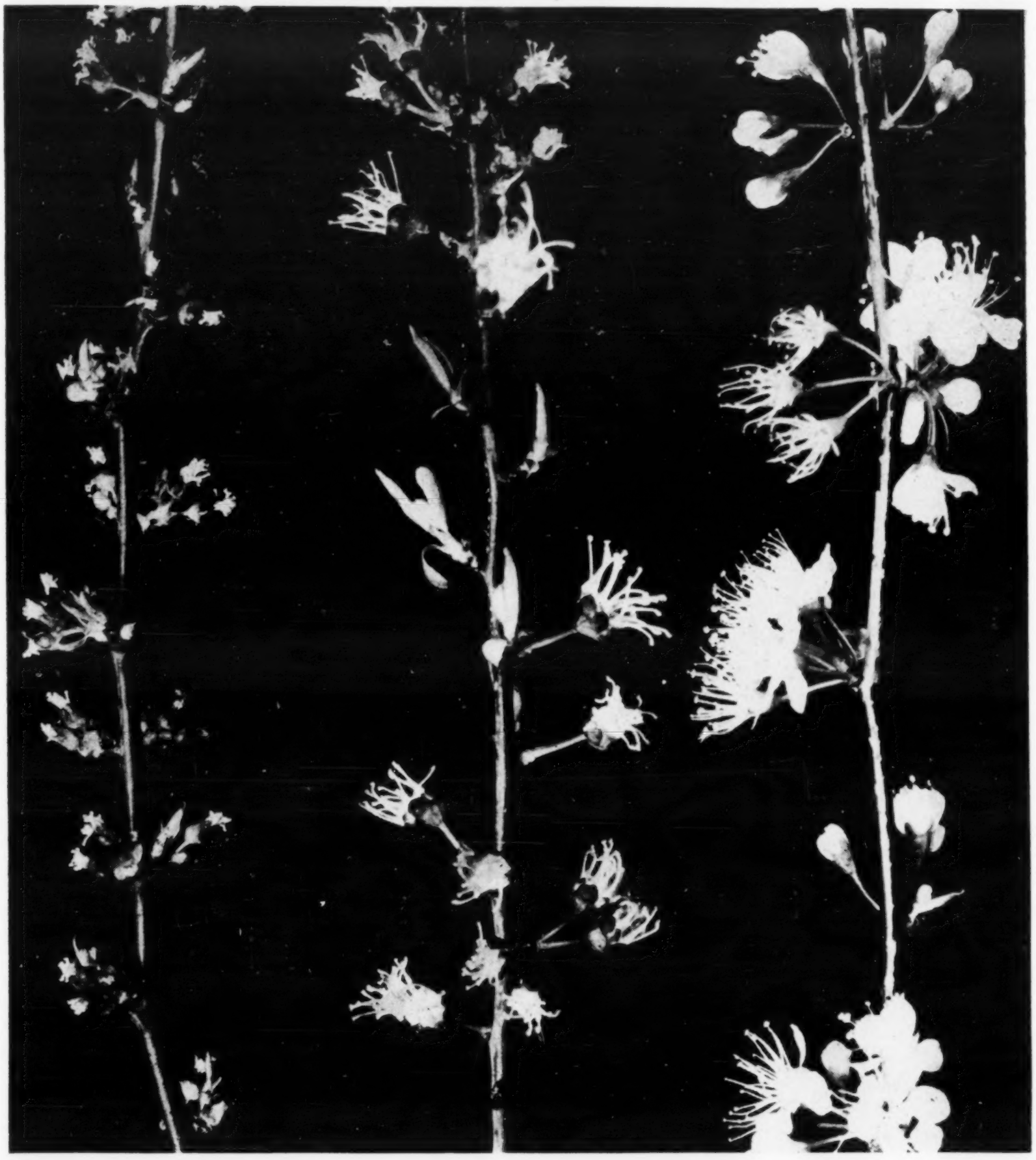
ENLARGED VIEW OF FLOWERS

Figure 9

Left, above, normal flowers of *Prunus umbellata*, natural size, below, enlarged about 2.5 times. At right, above, *Prunus umbellata* hybrid showing pistillate bloom, enlarged; below, enlarged about 2.5 times.

to be a typical specimen of that species. It has been under observation since planting in 1916, and has shown no indications of abnormality. It was sent to us by the United States Department of Agriculture, Bureau of Plant Industry, under the inventory number S. P. I. 38974. The seeds from which it was grown were from Raymond Robbins, Snow Hill, Florida, and were collected by William Gomme, at that time superintendent of the Field Station, Brooksville, Florida.

The study thus far has given no very definite indication of what has taken place to cause the modification



FLOWERS OF HYBRID

Figure 10

Right, normal flowers of *Prunus umbrella.* Left and center, pistillate flowers of hybrid, natural size.

in these seedlings, but it is most probable that they are accidental species hybrids. Most of the evidence seems to favor this conclusion. The flowers are sterile even when the pollen from the mother tree is used. The tomentose fruit and the shape of leaves suggest that the Japanese apricot, *Prunus mume,* may have furnished the pollen. Several trees of this species are growing nearby and are in flower at the same time as *P. umbellata,* which blooms at Riverside in February. The few *Prunus* forms that are in flower in the near vicinity at this time are saucer peach (Peento), *P. mume,* and an early-flowering form of *P. armeniaca.* Any of these three might have furnished pollen to *P. umbellata,* but the greater number of trees of *P. mume* would favor it as a pollen source.

If only one seedling exhibited the changed characters it might be considered as probably due to mutation, but as all of the progeny grown, 20 seedlings from seeds taken at random from the parent tree, showed the same

changed characters, it is thought most likely that they are accidental hybrids of the same parentage, which would be expected to produce nearly uniform plants in the F_1 progeny. The variation, whatever its cause, is a remarkable one, as the seedlings in flower would scarcely be recognized as belonging to the genus Prunus or to any other well known group.

A New Principle in Organic Evolution

BIOLOGY, like other sciences, progresses by means of the construction and demolition of theories. Many theories are condemned in the early stages but others reach a venerable age before finally crumbling from overelaboration. Most new theories are moderate departures from the accepted tenets, differing only in detail, but occasionally an entirely new conception is brought forth to be examined and tested in the laboratories of the world. Such a theory is Professor Wallin's Symbionticism.* Truly a formidable name but the idea for which it stands is even more heroic. Briefly Professor Wallin's thesis is that all the higher forms of life are compound organisms representing an inseparable association of bacteria with their host cells.

Symbiotic associations of two different forms of life are well known—the most common representative of this type is the lichen, where a fungus and an alga live amicably together. In the lower forms of lichens the component organisims maintain sexual independence, but this individuality is lost in the higher forms and the two members cooperate in the formation of the reproductive organs. Thus there is well-established biologic precedent for Symbionticism—the daring lies in the extension of the relationship to higher forms of life.

For some years the nature and function of certain microscopic bodies found in all cells and known as mitochondria have been receiving the attention of cytologists. Professor Wallin has observed that these bodies have many of the attributes of bacteria and he therefore considers them as such. If his conclusions as to the bacterial nature of mitochondria are correct it follows that all forms of life possessing these bodies are in fact symbiotic organisms. Though his contentions are certain to be viewed with skepticism,—if proved, they constitute a new epoch in biology.

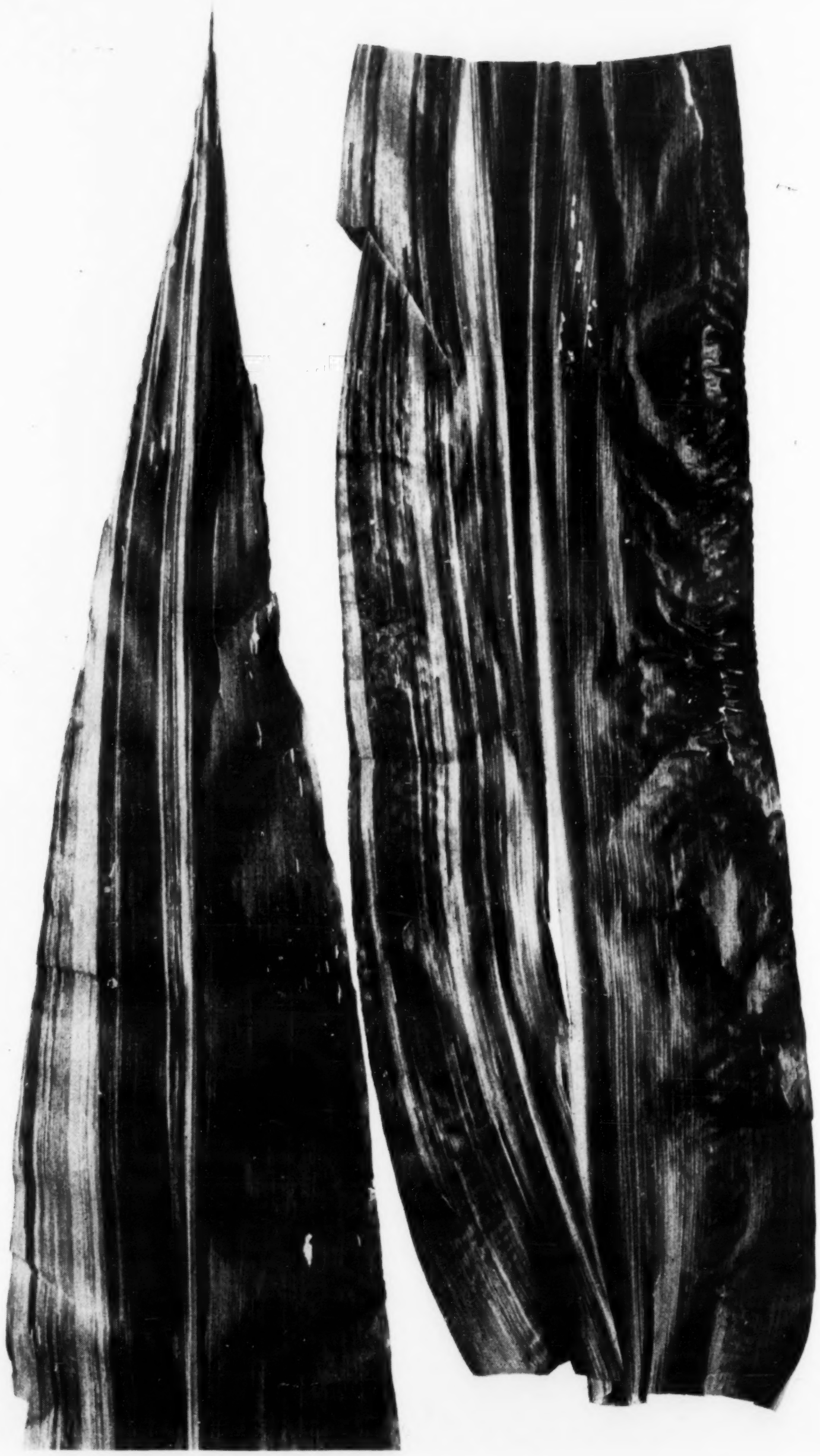
From the possible interaction of the included bacteria (mitochondria) and the host cells a multitude of reactions and variations can be conjured up. The bearing of such alterations, in structure and form, on evolution and heredity can be visualized readily and Professor Wallin discusses these features in an instructive though entertaining manner.

No one at the present time is in a position to affirm or deny the correctness of Professor Wallin's contentions, but the publication of his book should stimulate research in this field with the result that the truth or falsity of his position will be established eventually. In the meantime the book is of value for the novel viewpoint expressed and for its demonstration that biological questions of the most fundamental sort remain to be answered.

The book contains an index and bibliography and should be read by every one interested in the broader aspects of biology.

J. H. Kempton.

*Ivan E. Wallin. Symbionticism and the origin of species. 171 p. $3.00. Williams & Wilkins Co. Baltimore, Md. 1927.



A CHLOROPHYLL MUTATION IN MAIZE

Figure 11

Leaves of maize showing a sectional chimera. The original plant was normal green on one side and green striped with white of varying widths on the other side.

ANOTHER CHLOROPHYLL MUTATION IN MAIZE*

R. J. Garber and M. M. Hoover
Agronomy Department, West Virginia College of Agriculture

A FEW years ago it was pointed out by Kempton† that the large number of heritable variations in maize differing from the normal plant were usually recessive and of a degenerative character. A variation of this nature appeared (presumably by mutation) in 1925 in one of the writers' cultures of selfed maize. The particular strain in which the mutation occurred had been selfed for five years and was isolated from the Leaming variety.

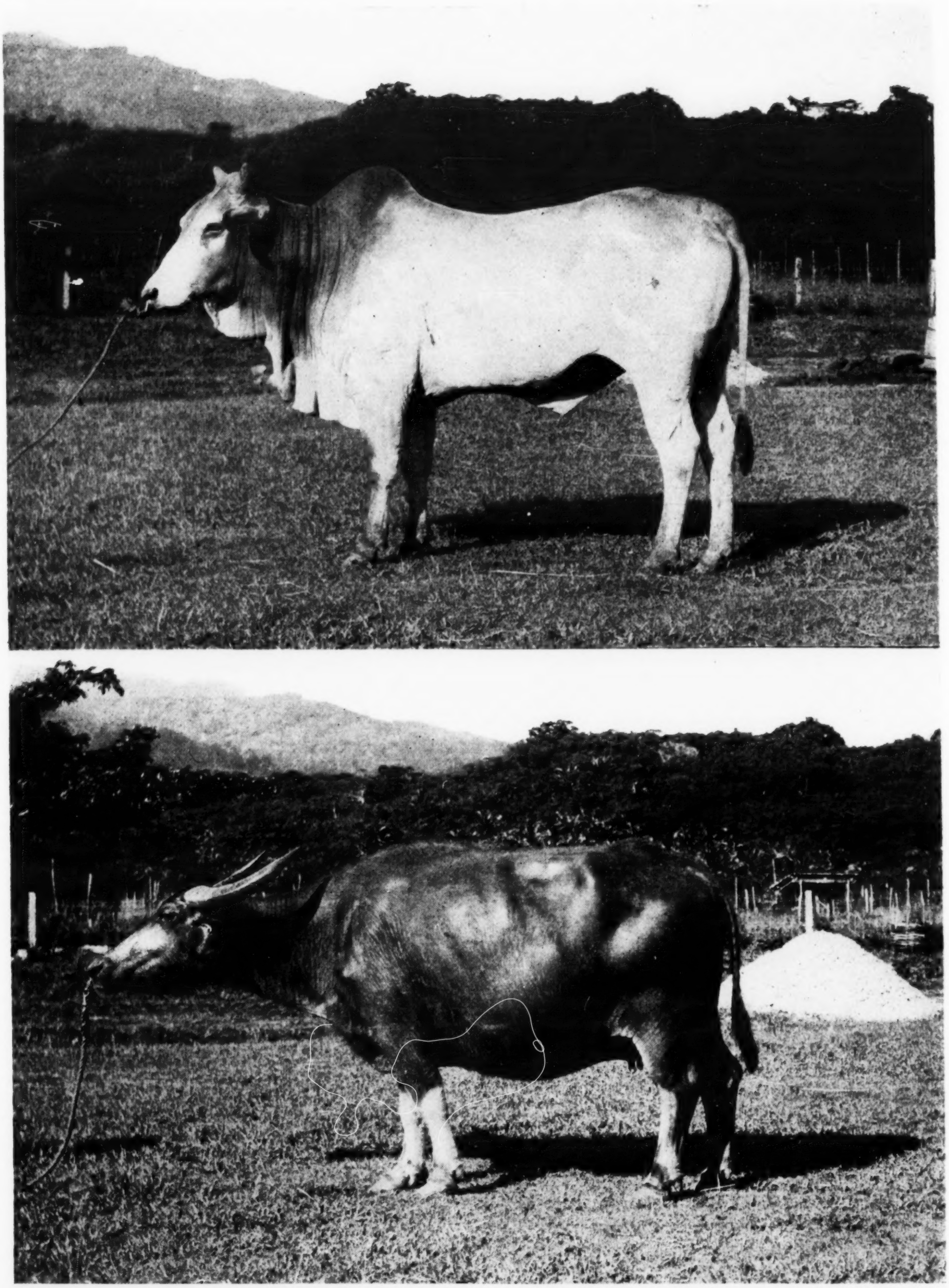
An idea of the appearance of the variation may be obtained by an examination of Figure 11. One-half of the plant was green and the other half was green with white stripes of varying widths. The striping was noticeable on the leaves and on the glumes in the tassel but only faintly so on the leaf sheaths and not at all on the stalk. The glumes on one side of the tassel were striped and on the other side they were normal in color. If one were to take a knife and split the plant from top to bottom following the mid-ribs of the leaves, one-half of the plant would appear normal and the other half would appear green with white stripes. The bilateral nature of this chimera is similar to those described by Khadilkar‡ and by Kempton, but it differs in coloring. The one observed by Khadilkar had yellow and whitish stripes and the one observed by Kempton had a solid yellow color.

The plant observed by the writers was selfed with pollen from the striped side of the tassel. The resulting seeds were planted in 1925 and produced nothing but normal green plants. Owing to an oversight none of these plants was selfed in 1925 but since they were grown in a rectangular block, seed from some of the inner plants was saved and planted in 1927. Of 209 plants 14 were striped somewhat similar to the striped half of the original plant. This seems to indicate that the variation was heritable and recessive and that either the male, or the female gametes of the original plant bore the recessive gene.

*Approved as scientific paper No. 37 West Virginia Agricultural Experiment Station, Henry G. Knight Director—August 31, 1927.

†Kempton, J. H. A dominant lethal chlorophyll mutation in maize. *Jour. Agric. Res.* 29:307-309. 1924.

‡Khadilkar, T. R. A sectional chimera in maize. *Jour. Hered.* 12:284. 1921.



PHILIPPINE ZEBU AND WATER BUFFALO

Figure 12

The Zebu, *Bos Indicus*, (upper figure) and the water buffalo are the principal draft animals of the Philippine farmer. Each has advantages that the other lacks, and a hybrid between the two might be of great economic value, but so far none have been reported.

IS A ZEBU-CARABAO HYBRID POSSIBLE?*

B. M. Gonzalez and J. P. Esguerra
College of Agriculture, University of the Philippines

TO anyone in the tropics familiar with the water buffalo, or carabao (*Bubalus bubalis*) as a work animal, with its merits and drawbacks, the question is likely to come: Is a hybrid zebu-water buffalo, a hybrid similar to the mule, possible?

The special merits of the water buffalo are its large size, which enables it to develop great power, the modesty of its feed requirements, as it consumes coarser feed than the ox and demands no concentrate, and especially its adaptability to lowland rice culture, with all the peculiar conditions incident to this crop. Its defects are that it is very slow in work, and what is more important, it tires easily when exposed to the sun and is very inefficient when used at all during the hottest part of the day. On the other hand, the merits of the ox, the work ox of the Philippine Islands, lie in the fact that it is a more rapid worker, capable of enduring long hours of exertion under a tropical sun. Its defects are its small size, which makes it correspondingly a smaller power unit, and that it is not adapted to rice culture. Considering that a large share of the agriculture of the tropics is rice cultivation, it may be realized that the ox finds little acceptance in the rice-producing regions.

Obviously, from the statements just given, the defects of the water buffalo are the very virtues of the ox, and *vice versa*. If it were possible, then, to combine the merits of the two animals into one individual, we would have an animal suitable for most activities of a tropical farmer; since a farmer, at least in the Philippine Islands, is often engaged in both upland and lowland culture crops.

Alleged Hybrids

Our first interest in the possibility of this cross was aroused by stories of two students in our former classes, one of them from Santa Cruz, province of Laguna, who told us that he had seen an animal about the town which was reputed to be a hybrid ox-carabao.† Another student from Naguilian, Isabela, related also that he had seen an animal which was said to be a mestizo ox-carabao. Both of those animals were described as having characteristics intermediate between the ox and the water buffalo.

The student from Santa Cruz, Mr. Victor Sulit, now an instructor in the College of Agriculture, gives us a more detailed description of the supposed hybrid, as follows:

Horns—short, drooping downwards and backwards.

Color—black hair, thick and shiny.

Size—lower than the water buffalo, but considerably wider than the ox.

Resistance to heat.

At a distance the animal looks like an ox.

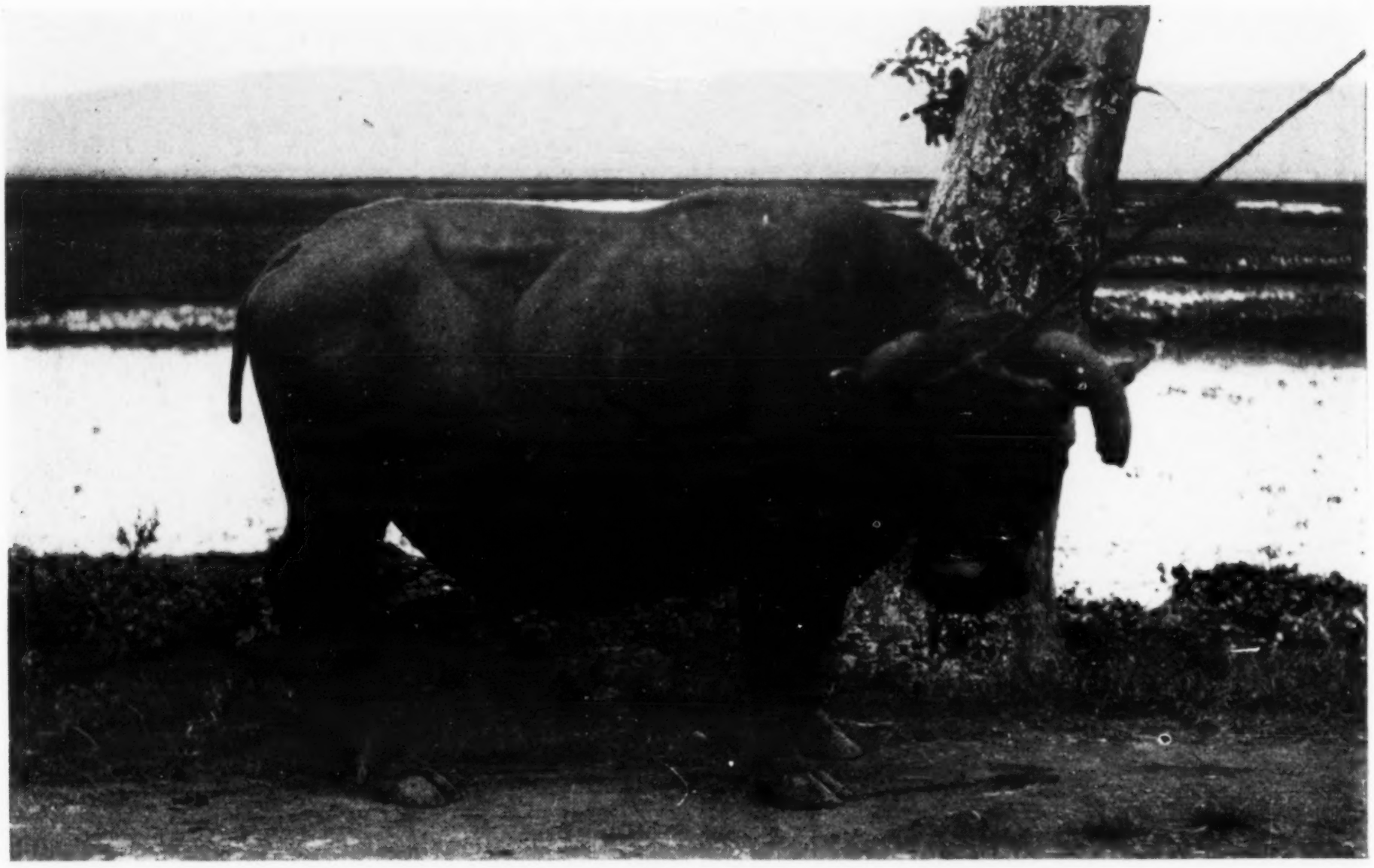
Animal claimed by owner to be stronger than carabao. This claim is doubted by informer.

The informer believes that the contention that this animal is a hybrid is tenable. Other people that saw the animal were not able to decide as to whether it was an ox or a carabao.

We were never able to see these animals, nor satisfy ourselves as to the

*Paper read September 26, 1926, before the Philippine Society of Technical Agriculturists, Los Baños.

†After this paper was written we made a special effort to see the animal at Santa Cruz, Laguna. We found it to be a pure carabao, more low set than the ordinary. The only other evidences of difference between it and the common Philippine water buffalo are the horns which are large and heavy and drooping downwards, instead of the usual kind which extend outwards and backwards, and the short tail which shows marks of mutilation. The animal is said to be an imported animal and therefore the modifications from the usual type are perfectly explainable on the basis of variety differences. (See Figure 13.)



ALLEGED HYBRID ZEBU-CARABAO

Figure 13

The animal at Santa Cruz proved on investigation to be a water buffalo. It was said to have been imported, and its slight deviations from the Philippine type were doubtless due to varietal differences.

genuineness of the claims made for them. We do know, however, that people are apt to be inaccurate in their nomenclature; for example, strangers visiting the campus sometimes remark on seeing our Indian water buffaloes or our hornless carabao that they look like oxen, just because they look different from the common carabao. It is a fact that the water buffalo of the Philippines is quite uniform in appearance; that is, there are no pronounced differences among individuals in the characters little affected by the environment, such as color of skin and hair, shape of the head, horns, general conformation, and the like.

Our interest in the question of this ox-water buffalo hybrid led us to attempt to produce such a cross. In the beginning we tried on several occasions to breed a grown-up zebu bull to carabao cows supposed to be in œstrum, but the bull, a sure breeder with normal cows, was absolutely non-responsive. We decided, therefore, to follow a more roundabout method. Upon the birth of an ox bull calf, on July 26, 1921, he and his mother were kept with the water buffalo milking herd. When the calf was weaned he was transferred to the water buffalo young stock herd, and thus grew with such animals quite apart from any animals of his kind. This animal, which we named Jengheiz, bears our herd number 26, and is a grade Nellore-Philippine bull. As his sexual instincts developed, he was observed to mate with his carabao herdmates, readily and frequently.

Our carabaos are kept in two herds, the milking herd and the heifers' and dry cow's herd. The carabao cows thus change from one herd to another, according to their condition. The milking herd is headed at times by a carabao bull, and at other times by an Indian buffalo bull. Until shortly before he was castrated, Jengheiz was the only male with the heifers and dry cows.

Most of the cows upon drying would naturally return to the heifer herd after being bred, but the young heifers came to sexual maturity in the herd headed by Jengheiz. Jengheiz was kept with the carabao heifers until he was three years and three months old. He, therefore, had considerable opportunity to mate and produce offspring. There was never an offspring produced by the herd that looked different enough from a normal carabao to be considered a hybrid. We produced calves from the old cows, but none from the heifers. Finally, as we were losing in time and in calves in the unbred cows, we decided to call off the experiment and castrated Jengheiz. A regular buffalo bull was then put at the head of the heifer herd. All the heifers previously with Jengheiz, about five in all, subsequently calved to this bull. Jengheiz's fertility was never tested with regular cows. It may be assumed that he was fertile, however, as infertility among bulls is of very rare occurrence here. In our twelve years of experience in this locality we have never known of an infertile bull.

The literature on carabaos or water buffaloes is meager, and the only reference to this subject we have come cross is from Hansen* who states that "Mating between buffaloes and domestic cattle is nothing extraordinary although there has not been an authenticated case of impregnation; hybrids are, therefore, unknown." Hansen makes mention of no specific case, nor the locality where his observations were made, but our experience in this case fully corroborates his statement.

Seedless Persimmons

Editor, JOURNAL OF HEREDITY:

Washington, D. C.

DEAR SIR:

A number of horticulturists are looking for a seedless American persimmon of good quality which may be packed like dates for the market. Persimmons, when chosen at the proper point of wilting and treated as dates or raisins are treated in California, may then become a valuable market crop. The so-called seedless varieties present a peculiar feature in the fact that this characteristic appears not to be constant. In some years fruits of pistillate trees standing near staminate trees, contain one or more seeds. In other years, under practically similar conditions, seeds are absent.

If any reader of THE JOURNAL OF HEREDITY happens to know of persimmon trees which bear seedless fruit and wishes to have the variety tried out, I would be very glad to carry on the experiment for northern types at Stamford, Conn., and Dr. J. E. Cannaday of Charleston, W. Va., would do the same thing for persimmons of the more southern group. Any variety which promises to be particularly valuable will be named after the finder.

Yours truly,

ROBERT T. MORRIS.

*HANSEN, J. 1922. Lehrbuch der Rinderzucht. 2nd and 3rd rev. ed. 647 p., 316 figures. Berlin: Paul Parey.

HEREDITY OF HABITUAL WANDERING

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WHEN Mrs. Johnson calls the police station early in the morning and says, "Johnnie didn't come home last night; will you please try to find him again?" we wonder why the road is so appealing to Johnnie. Is he trying to get away from some unpleasant situation at home? Usually nothing of this kind can be found. Even if his bed at home does not have on it a spring mattress, it is better than a delivery wagon or an improvised shack at the city dump. Then there are men who leave their wives without provocation; others who girdle the globe working at some job that could be done as well in their home county, girls who leave the dishes half washed, to be found hours later, after diligent search, in a distant part of the city.

Some young people have a desire to see the world while in their 'teens, but later settle down to a well ordered, settled, satisfied existence. Some men move to distant regions after careful consideration, having decided it is the better place to be. Many people are glad to get away occasionally for recreation, but just as glad to return home. Are all these forms of traveling prompted by the same cause?

The present study is based upon the cases of Davenport and of Williams, which are given in full by those writers, and twenty-six new family histories, personally obtained, making a total of one hundred fifty families. Most of the these new cases were ob- [redacted] Bureau of Juvenile Research, Columbus, Ohio. Boys and girls are sent here for observation by judges of Juvenile Courts, and complete records are kept. In addition to this information I went out and called upon the parents and guardians of nomadic boys and girls, learning everything possible about the other members of the family.

Definition

We believe the time has arrived that there is sufficient material at hand to formulate a definition of nomadism; which I have not been able to find. There are three characteristics of this trait which form the principal means of its recognition. Let us look at them separately.

1. The wandering is impulsive. The typical nomad does not set out deliberately. A man did not come home to dinner one day. After waiting a long time his wife went upstairs and found that his clothes were gone. He had rolled them up and had thrown them out of the window. He had had no trouble with his wife or with any one else; he simply had an impulse to leave.

A certain boy would be playing very pleasantly, when presently his aunt would look out and see that he was gone. When the police would find him he would be in a distant part of the city. When asked why he went away he would say, "I don't know. It just seemed I had to go." He had a good home and his aunt was interested in him so that he had no reason to desire a better place. If he were trying to get away he would make consistent efforts to get away and would not come back; but he merely followed his impulse to wander.

2. The wandering is unreasonable. In a normal individual his impulses, if foolish or harmful, are inhibited by his reason; but not so in the nomad, if he gets the impulse to go. Two boys

*This paper is an abridgment of a thesis, complete copies of which art on file at Ohio State University and at the Ohio Bureau of Juvenile Research, Columbus, Ohio.

whom I studied would tramp through the snow and the slush for long distances when they might have been comfortable at home. A study of family histories shows that it is quite the common thing to sleep at night in a deserted wagon or auto or in a shack, regardless of discomfort. Most nomads will steal or beg when hungry; and the meagerness of the fare does not influence their reason enough to make them give up wandering. The trait being of such an unreasonable nature, it is usually hard for appeals to the reason to produce any effect.

3. The wandering is habitual. It is quite normal for one who has been confined at his post of labor to enjoy a trip for a change; but having returned he appreciatees his comfortable home and is more content with his job for having gone.

But those having the nomadic trait are well represented by the girl of whom it is said that, "She is never satisfied except when she is on the go." A boy promised the judge to settle down; but was brought into court three times afterward for running away. Another boy has been taken home by the police forty or fifty times in the last five years. This traveling is attempted continuously in some cases, periodically in others.

4. We may say then that in our sense a nomad is a person who has an inborn factor which induces him to wander impulsively, unreasonably, and habitually. Those who travel for a reasonable purpose, to escape bad conditions, or merely during adolescence, are not included. However, in attempting to recognize nomads, one must bear in mind the influence of environment, and of other hereditary traits.

Hereditary Nature of the Trait

1. Other explanations are inadequate, conflicting, and anything but scientific. One mother explains her boy's behavior by a fall which injured his head when he was young. Dr. Goddard says that a large per cent of the feeble-minded children at Vineland are thus accounted for by their parents, but very few cases are really due to this cause. The brain is so well protected that the skull must be dented severely before injury to the mental faculties is likely to result. And the boy cited above was not even feeble-minded.

Dr. Robbins, chief in the work against venereal diseases, Ohio State Department of Public Health, stated that syphilis is not a cause of wandering. A physician at the University Hospital, who was recommended as having had experience with sleeping sickness, assured me that this can not be held responsible for the trait.

That nomadism is caused by bad home conditions is not tenable. Many of the homes which I visited, which also were reported upon by the Bureau, were above reproach.

In the records at the Bureau the wandering of one boy was accounted for by the fact that he had superior intelligence and initiative; of another it is stated that 'His behavior is typical of a feeble-minded child." The two boys acted alike. All the boys and girls at the Bureau have been tested by the Stanford-Binet test; and the wide variation in mental age among nomadic persons shows that there is no correlation between intelligence and nomadism. I agree with Judge Olson of Chicago that we are trying to explain too much by intelligence, and neglecting the emotions and affective traits.

2. Nomadism follows the Mendelian principles of heredity. In a family where some of the ancestors were wanderers, a part of the children may love to roam while others are attracted just as strongly by a settled life. Their environment is the same; but due to the segregation of the genes, their heredity is different. What a difference it makes in their behavior!

There also are clear examples of dominance. Settled existence is dominant over wandering. There is no clear case where both parents were nomadic

and some of the children failed to show the trait. However, Davenport lists 42 families where the parents were steady but of nomadic stock, and some of the children were nomads. Furthermore I have not found any nomad who did not have some nomadic ancestors, where the family history is well known.

3. Josiah Flynt (Children of the Road, in Atlantic Monthly, Vol. LXXVII, P. 69, 1896) considers certain children who go on the road voluntarily to be products of heredity because of innate skill in travel.

4. One of the strongest proofs of the hereditary nature of nomadism is an investigation carried on by Williams.* He made a study of boys who did and who did not show this trait, all of them being pupils in his industrial school. The results follow:

Families of 24 Nomadic delinquents.

No. on charts	440
Definitely reported on	312
Per cent nomadic	30.4

Families of 24 Non-nomadic delinquents.

No. on charts	638
Definitely reported on	318
per cent nomadic	1.2 (4 persons)

Method of Transmission

1. Evidence for sex-linkage. It is a striking fact that more male than female nomads are reported. Davenport† lists in the principal fraternities of his tables 168 male nomads and 15 female nomads. In the appendix of the present study there are listed 49 male and 20 female nomads. These facts point to one of three conclusions: nomadism is sex-linked, it is sex-limited, or else it is easier for us to detect the expression of nomadism in males, that trait itself being due to an independent factor.

2. Evidence for a sex-limited factor. The only evidence is the disproportion of the sexes. But it is not large enough. A few female nomads might be exceptions, but not so when there are nearly half as many women as men.

3. Evidence for an independent factor. The limiting influence is not biological but social. Were it not for the restraints laid upon women by society, as well as their natural physical handicaps, we should see just as many wandering women as men. Davenport cited the fact that gipsy women and others find it possible to live a nomadic life. But most women will not thus give up their standing in society.

(a) Boys inherit nomadism from their fathers, which would be impossible if it were sex-linked. Of the 24 families in Williams' study, the boy inherited nomadism from both sides in 9 famlies; from the father's side alone in 9 families; from the mother's side alone in two families; the ancestry in the remaining four families being imperfectly known. A careful examination of families 7, 17, and 23 shows that there is no doubt in these cases; for in none of these cases is there any nomadism on the mother's side, though full accounts are given.

Further examples of the same fact are cited in families 1, 3, and 4 of this paper. I made a special trip to see the step-father of the boys in one case, who confirmed what the mother had told me, viz., that there are no wanderers known among her relatives. The father and his brother both were wanderers.

(b) Dr. Davenport says in support of his theory of sex-linkage, "There is no clear case of a nomadic daughter whose father is known to be non-nomadic." Since that time at least two cases have turned up, viz., No. 2 and No. 23 of the present study. The former father I know from my own acquaintance to be steady and even slow. The latter was described by the

*Williams, J. Harold. *Journ. of Delinquency*, Vol. I, No. 4, p. 209 ff.

†Davenport, C. B. *Nomadism with Special Reference to Heredity*, Carnegie Institute of Washington, 1915.

regular workers of the Bureau of Juvenile Research.

Summary and Conclusions

This investigation, based upon a study of one hundred fifty families, twenty-six of which are original, shows that an understanding of nomadism is important in dealing with delinquency. There is need of further study to determine what can be done for children who have inherited this trait. How can we keep them from joining the sordid ranks of tramps and criminals? Some inhibition seems possible, but the extent to which this can be practiced has not been determined.

The following points seem to be established:

1. The outstanding behavior of nomads consists of wandering which is (a) impulsive, (b) unreasonable, and (c) habitual.

2. Nomadism is a hereditary trait.

3. This trait appears to be due to an independent, recessive factor, not correlated with intelligence.

Banana Plants Yield Most Food Per Acre of any Crop

THE world's leading food crops, including wheat, rice, corn, and potatoes, have become great staples because of their large yields of energy-producing foods in concentrated and palatable form. In a comparative study of these foods, the banana, it is said by a representative of the United States Department of Agriculture in Hawaii, has been found to lead in total production per acre and in fuel value per acre. A fair yield of wheat is estimated at 1,620 pounds per acre. In the banana it is 32,000 pounds per acre. And the comparative fuel values from an acre are 2,673,000 calories for wheat and 8,320,000 for bananas. Of rice, corn, white and sweet potatoes the acre yield of sweet potatoes, with 6,160,000 calories, approaches closest to the banana. Of the fruits the banana is much the richest in protein, having approximately four and one-half times as large a proportion as the apple and nearly five times the pineapple.

For many races of man the fruit of the banana in great part takes the place of wheat, rye, barley, and potatoes. Flour is sometimes made from the green fruit. Banana flour is richer in protein than potato flour. In food value it is almost equivalent to wheat flour and corn meal.

From the plantains, which are banana-like plants, the fruit of which must be cooked before eating, a flour is obtained which is sometimes used in making delicate puddings.

Another relative of the banana is the abaca plant, from which manila hemp fiber is obtained. The fruit of the abaca is inedible.

—*U. S. Dept. Agriculture.*

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